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EYE-MOVEMENT DURING FLUCTUATION OF ATTENTION.[1]

By H. S. Liddell

Fluctuation of attention has been the subject of a great amount of experimentation and discussion. Theories are numerous and diverse and in spite of the increasing bulk of work done in the investigation of the phenomenon no theory has been generally accepted. One of the more recent systematic investigations of the attention wave was carried out by C. E. Ferree. In two articles published in this JOURNAL[2] he proposed a peripheral theory which, because of its plausibility and the quantity of data adduced as evidence for it, presents a challenge to the holders of other theories. His contention was that fluctuations of attention "are in reality, simply adaptation phenomena somewhat obscured by special conditions. Adaptation is, itself, a continuous phenomenon, but its continuity is interfered with by eye-movement, blinking, etc. Through these influences, probably essentially through that of eye-movement alone, it becomes an intermittent process, whether the stimulus be liminal or intensive, provided that proper areas be used."[3]

[1] From the Psychological Laboratory of the University of Michigan.
[2] C. E. Ferree: (1) An Experimental Examination of the Phenomena Usually Attributed to Fluctuation. Am. J. of Psych. XVII, 81 and (2). The Intermittence of Minimal Visual Sensations. *Am. J. of Psych.* XIX, 58.
[3] Ibid., (1) 83.

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Taking Ferree's position as a typical example of peripheral theory, the present research was undertaken with the purpose of testing his theory by investigating in a more exact manner than he had done, the eye movements occurring during fluctuations of attention. This problem was suggested by Professor Pillsbury whose constant aid and advice have been indispensable.

A formal statement of Ferree's thesis is as follows: "(1) The intermittence of minimal visual sensation is a phenomenon of adaptation. (2) Adaptation is rendered intermittent chiefly through the influence of eye-movement. (3) Eye-movement interferes with adaptation in three ways. (a) It decreases the total time of stimulation. The more eye-movement there is, the less intensive will be the impression made upon the retina. (b) It affords time for the after-image to die away, or (in terms of adaptation) it gives opportunity for restoration, proportional to the length of time during which the stimulated area is relieved. And (c) more immediately, it determines or influences the washing or streaming over the retina of some material capable of directly affecting the visual processes."[4]

If one accepts the evidences which Ferree adduces the thesis logically follows; however, the method employed in securing the crucial data does not seem adequate. The crux of the whole problem is the question of the distribution and extent of eye-movement during the fluctuation of the minimal visual sensation. To determine this matter Ferree used the following arrangement. "The shifting of the negative after-image during fixation afforded a method somewhat rough, but adequate for our purpose. Colored strips, 5 mm. x 40 mm., were used as stimuli. They were pasted on a background of white card-board, with the shorter dimension in the plane in which the eye-movement was to be investigated. The determination of frequency then became merely a matter of recording the appearance of the after image to the right or left or above or below the stimulus, separate series being made for both planes. For the determination of range of movement, narrow strips of paper of the same brightness as the background were placed successively 2, 4, 6, 8, etc., mm. from the stimulus, and only those movements recorded that caused the after-image to shift to or beyond these strips. The strips were so inconspicuous as not to attract the eye away from the fixation point; still, it was not difficult to judge when the after-image reached, or passed beyond them. Some

[4] Ibid., (2) 129.

periods were given up wholly to the investigation of eye-movement alone, thus determining the type in general; while again the eye-movement tracing was alternated with the corresponding fluctuation tracing, in order to establish a more immediate connection between the eye-movements in either plane and the phases of visibility and invisibility in that plane. Doubtless it would have been better to have the eye-movement recorded while the fluctuation was in progress could this have been done without interfering with the normal course of the phenomenon. As it was, however, enough results were obtained to render conclusions safe as to the type of the observer."[5]

What Ferree regards as a slight disadvantage, i. e. the inability to record eye-movement while fluctuation was in progress, seems quite clearly to offer a difficulty which must be overcome if his theory is to be put upon a convincing basis. Before the casual relation of eye-movement to the fluctuation of attention can be established the temporal relations involved must be definitely determined. The most that can be done by Ferree's method of approach is to obtain suggestive approximations. In a problem of this kind there can be no substitute for simultaneous records of eye-movements and fluctuations of attention. Now Dodge's method for photographic registration of eye-movements can be employed without interfering with the normal course of the fluctuation of attention. The present investigation is a study by photographic registration of the eye-movements occuring during the fluctuation of attention and was carried through with the conviction that Ferree's thesis must stand or fall on the evidence supplied by satisfactory simultaneous records of fluctuations and eye-movements.

A full description of the apparatus is unnecessary since a detailed statement of the technique is to be found in Dodge's study of visual fixation.[6] A few sentences on its general arrangement will suffice. The experiments were performed in a dark room. The observer was seated with head held immovable in a head rest providing a support for the forehead and a rigid mouthpiece of wood and sealing wax to be gripped by the teeth. Directly before him at a distance of 169.5 cm. a square of light was exposed, both area and intensity of which could be varied. The intensity could be controlled by the observer himself. The area used, unless otherwise indicated,

[5] Ibid., (1) 113.

[6] R. Dodge: An Experimental Study of Visual Fixation. *Psych. Rev. Mon. Supp.* VIII, 4.

was 1 cm. At the observer's right an arc lamp projected a beam through a needle-point aperture, a plate of blue glass, and a system of lenses focussing upon the cornea of the left eye at an angle of approximately 30° to the line of sight. The reflected beam entered an enlarging camera and fell upon a photographic plate in a Dodge falling plate device. The rate of fall of the plate varied in these experiments from .8 cm. to 2.38 cm. per second. The form of the eye record was a straight line on the plate when the eye remained motionless. Horizontal or oblique eye-movements produced horizontal or oblique deflections of this line. The observer recorded the appearances and disappearances of the fixation light by means of a telegraphic key in circuit with an induction coil, the secondary terminals of which led to a sparkgap mounted in a tube closed at one end and fitted at the other end with a short focus lens. The tube was so adjusted within the camera box that a tap of the key caused a spark to record itself on the photographic plate at the side of the spot of light from the eye. A single tap of the key signaled an appearance or the beginning of a visibility phase, two taps signaled a disappearance or the beginning of an invisibility phase.

A few remarks are necessary concerning the control of conditions during the experiments. In the first place, as has been indicated, the observer controlled the intensity of the fixation light. No attempt was made to keep this intensity constant from experiment to experiment but only to make certain that the fluctuations should occur as regularly and as clearly as possible for each observer. Since all observations were made in the dark it was necessary to allow the subject's eyes to become dark adapted before any records were taken. A further interval was allowed in which the subject adjusted the fixation light and practiced recording the fluctuations. Since the arc lamp was in operation during this time the conditions were exactly those under which the records were later taken. In addition to this practice period at the beginning of each sitting at least one entire preliminary sitting was allowed for practice, the first records being taken the day following or in some cases two days later.

Distraction was eliminated as far as possible. The humming of the arc lamp was soon ignored while the noise of the induction coil vibrator was reduced by enclosing the coil in a box. To determine the extent of the distraction caused by the signaling itself two controls were run for each subject. First, the intensity of the fixation light was increased until no fluctuations occurred and a record was obtained in which the ob-

server signaled at a tempo approximating that of the usual periods of fluctuation. The effect of distraction which we wished to observe, if present, would be indicated by marked horizontal displacements of the approximately straight line in the record of the eye during fixation, these displacements corresponding to the positions of the signals. None of these records showed appreciable movements of the eye at the moments when the signals were given. Secondly, a set of controls was run for each subject in order to determine whether the eye-movement record during fluctuation would be modified were signaling abandoned. These controls were also negative.

Fatigue presented no problem since its onset could always be detected by the experimenter, for when fatigue was pronounced the beam reflected from the cornea would persistently drift out of the field of the camera lens, necessitating repeated readjustments of the apparatus. The observer was in each experiment cautioned to report fatigue as soon as it was noticed and in all such cases the experiment was at once discontinued. It ought also to be mentioned that each observer was thoroughly informed as to the nature of the problem and of the operation of the apparatus. This was deemed necessary in order to secure the fullest coöperation of the subject during the course of the investigation. The records were taken on 5x8″ rapid dry plates, two records on each plate. From 4 to 12 plates were taken at a sitting depending on the capacity of the subject. The records from each sitting constitute a series. In calculating the results each series was treated separately.

It now remains to describe the procedure followed during a typical sitting i. e. in obtaining a series of records. The room being darkened the observer was seated with head firmly fixed in the head rest. The arc lamp was set in operation and the necessary adjustments of the apparatus made. The observer then varied the intensity of the fixation light until it appeared to fluctuate clearly and regularly. He signaled each appearance and disappearance thus enabling the experimenter to decide when the fluctuations were occurring with sufficient regularity to begin recording. At the proper time the experimenter indicated by the word "now" that a plate was being exposed. The end of the exposure was similarly indicated. Immediately a second exposure of the plate was made and at its conclusion the introspections for the two records were given. After each two records and while a new plate was being prepared for exposure the observer was allowed an interval of rest. (Usually from 12 to 14 records were taken at a sitting.) The controls

were usually run at the end of the series though this rule was not invariably followed.

Results were obtained from the following seven subjects: Professor Pillsbury (P), Dr. Benedict (Bd) Head of the department of Ophthalmology in the Mayo Clinic, Rochester, Minn., Mr. Ross (R), Mr. Breininger (Br), Mr. Stayer (S), Miss Gordon (G), and Miss Wylie (W), students of psychology. Messrs. Cooley and Bowen, and Miss Wylie gave much time and patience in serving as subjects in the preliminary stages of the work. W supplied several records of eye-movement during fluctuation without signals. These records are of value in showing the type of eye-movement in her case. From R 4 series were obtained including the records of about 80 fluctuations (counting the number of fluctuations for each series as one less than the number of appearances), from Bd 3 series or about 48 fluctuations, from P 3 series or about 30 fluctuations. In the case of P one series of records was destroyed before the results could be computed. The character of the records of this series was observed to be the same as regards type of eye-movement as for those of the remaining two series. The records from Br, S, and G were taken merely to confirm the general conclusions rendered certain by the data from R, P, and Bd. From Br 2 series or 13 fluctuations were obtained; from G 1 series or 13 fluctuations, and from S 1 series or 5 fluctuations.

The scope of this investigation has been quite rigidly limited to a study of the presence or absence of eye-movement during the fluctuation of attention. We desired simply to know whether or not eye-movements regularly accompany the fluctuation of attention, and if they do, whether or not they regularly occur at or near the beginning of the visibility phase.

The subjects showed two distinct types; first, those who exhibited frequent eye-movements during the fluctuation of attention. Of these again there were two kinds. The eye-movements were either evenly distributed over both the visibility and invisibility phases or they occurred mainly during the invisibility phase. In the second type of subjects the eyes remained relatively fixed during the fluctuations of attention. When movements of the eyes did occur, they occurred at random.

Let us consider, first, the data obtained from R. An examination of Plate I, R_1 and R_2 will show that in his case with the onset of the invisibility phase the eyes exhibited large and frequent movements which were discontinued at the return of the visibility phase, or soon after it. These movements

were seldom less than 85′ of arc and on the average (as may be seen from a later table) ranged from somewhat less than 130′ to movements exceeding 300′. Any one of these movements of R's eyes during the invisibility phase would have been sufficient to cause the return of the visibility phase were the fluctuations really causally dependent on eye-movements since the arc subtended by the fixation light on the retina under the conditions of the experiment was but 20′.

The introspections given by R were illuminating. R was convinced that in his case "eye-movement caused the return of the visibility phase." The invisibility phase was a period during which a "sense of effort was experienced" and "this effort seemed effective in bringing back the light." The disappearance of the light, on the other hand, was a "passive experience." Of course, it can be seen from the records R_1 and R_2 that the eyes did move during the invisibility phase but they moved a number of times. R's introspections contain, however, no detailed account of how large or frequent were the eye-movements but only that the eyes did move and that "as a result" the light returned. The form of record shown in the plate persisted essentially unchanged until the fourth series. Here the group of movements so characteristic of R's invisibility phases showed a gradual reduction in range and number. This might have represented a transition in R's type of eye-movement during fluctuation of attention. However, speculation is useless as the records had to be discontinued at this point.

P, also, exhibited eye-movements during fluctuation of attention. However, a comparison of P_1 and P_2 with R_1 and R_2 (Plate I) shows a fundamental difference. In P's case the movements occurred at fairly regular intervals and were distributed evenly over both visibility and invisibility phases. Moreover the type of record shown in P_1 and P_2 persisted throughout the three series of records without any essential modification.

P's introspections also differed from R's in an important respect. No particular effort or strain was noted as occurring during the invisibility phase although the eyes were constantly in movement at this period as in R's case. An interesting experiment was performed during one of P's sittings. Records were taken when he endeavored to keep his eyes fixed on the light and to prevent, as far as possible, all eye-movement. Other records were taken in which no attempt was made to influence eye-movement. The effect of this attempted control of eye-movement during fluctuation is apparent if one com-

pares P_1 with P_2. The type of record in the two cases is the same, but in P_2 the eye-movements seem to be restricted as compared with those in P_1. It was impossible for P to modify essentially his type of eye-movement during fluctuation of attention.

The following table gives the records from R and P, two series each, tabulated for comparison. The values given are, of course, averages. In computing, 1/5 second was allowed for reaction time in determining the positions of the signals with respect to the eye record line. This correction does not appear in the records reproduced in Plates I and II.

Subject	Vis.	Eye-movement		Invis.	Eye-movement	
	sec.	No.	Range (mm)	sec.	No.	Range (mm)
R	2.92	1	3.2	2.05	3	1.2—3.2
R	2.84	1	3.7	2.07	2	1.4—2.9
P	2.37	4	1.0—1.9	2.16	3	1.0—1.8
P	1.81	3	1.1—1.5	2.13	3	0.9—1.9

The magnitude of the eye-movements is to be found in the column headed "Range" and is given in terms of the lateral displacement of the eye record line measured in millimeters. The range represents the average of the smallest and the average of the largest movements occurring during the visibility and the invisibility phases respectively. In order to translate these displacements in millimeters into eye-movements expressed in minutes of arc five determinations were made. Records were taken with subject P when he moved his eyes to and fro between fixation points separated by 2, 4, 6, 8, and 14 cm. respectively and at a distance of 162 cm. from the observer. These determinations are as follows:

Displacement of the line in mm.	Eye-movements in minutes of arc.
0.8	42
1.0	86
1.3	130
2.1	170
2.5	296

Take, for example, the value 3.2 mm. from the "Range" column above. This corresponds to a movement exceeding 296′ of arc. It will be remembered that the fixation light at a distance of 169.5 cm. from the observer subtends an arc of approximately 20′ on the retina. Of course, interpolation is unsafe in changing millimeters to minutes of arc but these five determinations will serve their purpose in showing that for

PLATE I.

Note: Plates I and II are to be read from the bottom upward. The signals are at the right of their corresponding lines. One dot signals the appearance, two dots the disappearance of the fixated light. The lines of signal dots have been adjusted to correspond accurately to the eye-movement records.

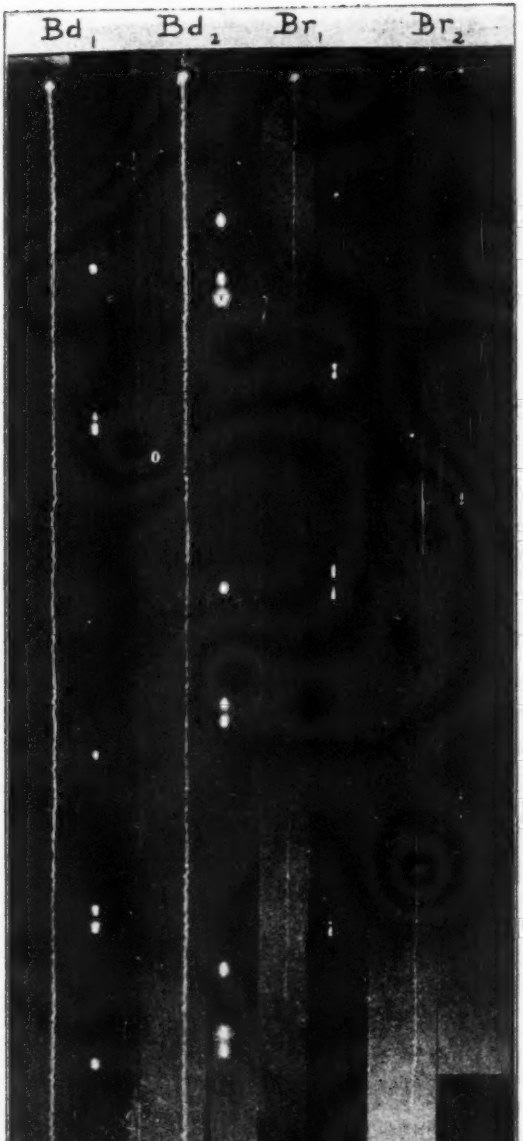

PLATE II.

both P and R eye-movements regularly occur during the invisibility phase which are much larger than those required by Ferree's thesis and yet two or three such movements do not suffice to cause the recurrence of the visibility phase. The records of S and W are similar in a general way to those of P; i. e. eye-movements are more or less evenly distributed.

We come now to the second class of subjects. In the cases of both Bd and Br the eyes remained relatively fixed during fluctuation of attention. This is clearly seen if one compares Plate II with Plate I. To be sure, the lines Bd_1, Bd_2, and Br_1, and Br_2 present a wavy appearance, but this is due, as Dodge found, to pulse and head movements.

Bd's eyes were even steadier than Br's, in whose case a few random movements did occur. Bd had remarkable control of his eyes. Early in the experiments he reported a strong tendency toward "searching movements" during the invisibility phase and he believed that when he allowed himself to make these eye-movements that they "caused the light to reappear" and that refraining from them lengthened the invisibility phase. A special group of records was therefore taken. In certain of these records Bd was instructed to make the searching eye-movements. In the other records he maintained as rigid fixation as possible. Unlike P, Bd was able actually to control his eye-movements during fluctuation of attention. Plate I, Bd_3 shows a record of his eye-movements during fluctuation of attention when the "searching movements" were voluntarily made. We have here a type of eye-movement resembling that of R and disproving Ferree's thesis just as definitely as do R's records; for large eye-movements at the beginning of the invisibility phase do not cause the immediate onset of the visibility phase.

On the other hand the cases in which the subject's eyes remain relatively fixed, viz. Bd_1, Bd_2, Br_1, and Br_2 are just as disastrous for an eye-movement theory of the fluctuation of attention since no prominent eye-movement occurs in these cases at, or near the beginning of, the visibility phase.

In Br's case, voluntary control of eye-movement during fluctuation was even less noticeable than in P's records. Br's eyes remained fixed during both rigid fixation and relaxation, i. e. when the subject observed the fluctuations of attention in a passive manner. However, Br's attempt voluntarily to maintain rigid fixation during fluctuation exhibited itself to introspection in a curious fashion. In such cases the reappearance of the light after invisibility came as a shock, similar, as Br described it, to the experience of one riding in a train which

has entered a tunnel and then after an interval suddenly emerges. Br also noted a slight tendency during the invisibility phase to try to "bring the light back." G's records were of the same general type as those of Bd and Br.

Two further points should be mentioned. The average fluctuation times for the subjects Bd and Br were as follows:

Bd when eyes moved.....	av. vis. 2.01 sec. av. invis. 2.68 sec. for 4 fluctuations.
Bd no eye-movement.....	av. vis. 3.47 sec. av. invis. 5.83 sec. for 24 fluctuations.
Br.....	av. vis. 8.69 sec. av. invis. 5.83 sec. for 11 fluctuations.

The individual differences were so great and the cases so few that no inferences have been drawn from the durations of visibility or invisibility phases under the various conditions and among the various subjects.

The effect of variation of the area of the fixation light on eye-movements during fluctuation of attention was determined for subject Bd. The type of eye-movement exhibited by Bd remained essentially unaltered when 4 mm.^{2} and 9 cm.^{2} areas were employed in place of the usual area of 1 cm.^{2}

In conclusion, we can say that fluctuations of attention are not causally dependent on eye-movement; for (1) some subjects exhibit eye-movements during fluctuations of attention and these movements occur during the invisibility phase without causing the immediate recurrence of the visibility phase and (2) some subjects show little or no distinguishable eye-movement during fluctuation and where the eye-movements do occur, they occur at random. No prominent eye-movements regularly occur at, or near the beginning of, the visibility phase. These results are incompatible with the assumptions of an eye-movement theory of the fluctuation of attention.

WHAT IS "THE UNCONSCIOUS?"

By Henry Jones Mulford, M. D.

When Freud presented "the Unconscious" to us he uncovered a mystery. That is, he uncovered our ignorance, for, what is mystery but ignorance? He brought home to us the fact that we were in a lamentable state in regard to our understanding of the human mind. We always had looked upon the human mind as a thing marvellously subtle, uncanny and difficult, and Freud but confirmed our belief. The very phrase "the Unconscious" was mysterious in our sight, and we stood aloof and gazed at it with wondering eyes and a doubtful mind. We always had suspected that there was something awful in the human mind, and now we were sure of it!

And why were we sure? Was it not because of the very word "Unconscious"? Mind itself, being a mystery to us, this further word but confirmed that mystery. The two words became associated in our minds, and, with the mystery suggestion in the background, no other result could follow. But, the high priests of the Cult tell us, we committed an error in thus associating these words. While we may say "the Unconscious" we may not say "the unconscious mind." The Unconscious may do this and may do that, *may behave as mind,* but it is not mind. Naturally the situation was confusing; it especially was confusing since others were using at this moment those other mysterious words, "subconscious," "co-conscious," and "subliminal," in association with the word "mind."

It is not at all surprising, then, that we were all at sea over this situation. One would have to have a steady mind in order to keep it steady under the circumstances! How could the machinery of the mind be used as mind and still not manifest mind? The high priests agreed, in forbidding the association of the word "mind" with the word "unconscious," that it could not; and then asserted, in recording their observations, that it could. What could a poor ignoramus do then? What was there left but mystery?

But now, why the mystery? Is not the human mind understandable? Of a truth it is. No matter how varied its phenomena may seem to be, no matter how startling, no matter

how puzzling, it may not be impossible to classify them. All that is needed is an understanding of the machinery through which the phenomena are manifested. And for this machinery we do not have far to seek Not farther than the human brain, for the human mind is manifested through the human brain; the machinery of the one is the machinery of the other. Any mind phenomena, then, must be referred to the brain if they are to be understood. And, coming to "the Unconscious," that, too, must be referred to the brain, for, whether it does or it does not belong with the mind, *it is observed in the region of the mind;* its phenomena arise through the machinery of the mind. It is, then, to the brain that we must go if we would understand "the Unconscious."

At first glance the human brain does not appear difficult, its machinery being, to the naked eye, a simple arrangement of cells and fibres, fibres that transmit stimuli and cells that receive and respond to the stimuli. But the arrangement is not simple, however it may appear. There is something here beyond the reach of the naked eye. *What is the reaction within the cell?* What is it that enables the cell to respond to stimuli? It is not difficult to understand how the cell receives the stimulus, for that is, after all, in line with its transmission over the fibre, a continuation of the cell: but, how is the cell, having received the stimulus, able to send back a reply?

The nerve fibres have bothered us some, but that was because of their complex distribution. But, having traced these fibres from their cells of origin to their areas of distribution, we now have a fairly clear idea as to them. The main difficulty for our human understanding lies still within the cell itself. Why does the cell react to stimuli? And why is the reply not always the same? Why is the reply sometimes so aimless and so inconsequential? Here is where the mystery begins to appear. There seem to be two levels of activity here, one, free and intelligent, one, restricted and uncertain. And, another point, not only must we consider the reaction within the cell itself, but we must consider also that reaction in its relation to things extraneous to the cell. In their order, then, we have these to consider: The stimulus arising outside of the cell and sent in to it over the nerve fibre; the reaction within the cell itself; the interaction between one cell and another; and the final manifestation of the reaction. There are here three phases, action, reaction, interaction, but of these the reaction phase, because it occurs within the cell itself, is the most important and the least understood. We seek a word that will designate it but what word is there? Does the word

"conscious" fit the situation? Does "unconscious"? If the reaction is conscious then why is its manifestation so often so bizarre, so often apparently without consciousness? If unconscious, then why the so constant evidence of consciousness? The fact is, the reaction within the cell is itself a reaction-complex, and, therefore, beyond the reach of any single word. In order even to approach to an understanding of this reaction it will be necessary to know something of the history of the cerebral cell, to know from what ancestors it came, and its method of development from those ancestors. Cell reactions may be interpreted in terms of cell phylogeny.

The primitive cell contained the primitives of human behavior. It sustained itself through the taking in of food and oxygen; its fluids circulated and it threw off waste; it had offspring; and it reacted to its environment. Today we observe all of these reactions in the primitive amoeba, and, taking this one-celled organism as an example of a primitive cell, we ask ourselves the question; Does this cell know what it is doing? Does it possess consciousness? The answer to that question will follow our understanding of the word "consciousness." If by consciousness is meant intelligent self-direction, the ability of the cell to direct the reaction between itself and its environment, we must deny consciousness to the cell; but, if by consciousness is meant merely the ability of the cell to react to external stimuli, then we must acknowledge that the cell does possess it. The cytoplasm of the primitive cell possessed the ability to react only to touch. The cell could not originate a stimulus, nor could it give intelligent direction to its reply; it could not vary the reaction, it could do only what it was told to do. It could react only to an external stimulus. Its consciousness was, in short, nothing more than reflex consciousness. Reflex consciousness is reaction to contact, without knowledge of the reaction before the contact, or knowledge of it after. The independent cell cannot perceive the reaction approaching, nor can it consider the reaction after it has passed. The cell hasn't the power to consider, it has only the power to respond. The primitive cell constantly was responding to stimuli; environmental stimuli pricked it upon all sides. Its primitive functions were reflex manifestations; it performed them in response to activating stimuli. Contact set the machinery in motion, and the machinery knew how to act when it was told to act. This consciousness, if it may be called that, was not conscious consciousness; it was merely reflex consciousness. It may be said, perhaps, that this reflex consciousness was the beginning of conscious consciousness.

All cells of the animal organism are descendants of the primitive cell, and, as descendants, partake of the nature of their ancestor. All have this reflex consciousness. Even the cerebral cells have it, in the lower animal as well as in the human animal. The primitive cell was an outside cell in all its aspects, it was in direct contact with outside environment upon all sides; and the cerebral cell, having been developed from the ectoderm, is in its origin an outside cell. Before it became a cerebral cell it was an outside cell responding to external stimuli; and, having acquired the habit of reflex response through long ages of exercise of that habit, the habit persisted even though the cell had changed its location and had taken on new functions. The cell, as a cerebral cell, was entering a higher phase, but the attributes of the lower phase still remained a part of it. The reflex still remained its dominant characteristic. And it was impossible to have it otherwise, for the reflex was basic, was inherent in the cell protoplasm. But, while it was basic, the apparatus over which it manifested itself had become more complicated. The cerebral cell was no longer in direct contact with outside environment. It now was only one of a vast number of cells of the same character isolated from the outside world within a circumscribed bony cavity. The cerebral cell had been taken out of the world, as it were, but, even though it was out of it, it was not out of touch with it. The axon which the cell had developed for the purpose kept it in touch with the environment of the individual. But now, while this cell still remained a reflex cell, it was taking on a higher faculty. It now was becoming a cell that could act by itself. It did not need an external stimulus; it could originate the stimulus. The cerebral cell was developing conscious consciousness. We now begin to sense the complication here. The cerebral cell was manifesting two functions over the same apparatus, the one old, inherent, the other new, a product of development.

We see, then, that the cerebral cell has expression in two directions; the one a touch-and-go reaction, a flash, a quick, unconsidered response to an external stimulus; the other a slower reaction, a reaction in which the stimulus is received. *considered,* and a reply returned that will best suit the situation. This cell has memory and initiative, it can think for itself. The primitive cell had memory but it did not have initiative, it could not think. The cerebral cells having developed the power to think consciously, now are able to direct the behavior of the individual without the aid of external stimuli. These cells do not have to be told what to do, *they know what*

to do. But, at the same time, their primitive habit of reflex action often stands in the way of a proper response to stimuli. While the cells are able to express themselves consciously, the expression often follows through the reflex, becoming quick and aimless and without real value to the individual. It is, in the ultimate, nothing more than automatism. This would be automatic thought, not real thought. It manifests itself through the machinery of thought, and is, therefore, thought; but it is not conscious thought, thought in which the subject is considered upon all sides, it is merely one-sided, reflex thought. The cerebral cells, specialized as thought cells, are behaving as contact cells because they were contact cells before they were cerebral cells. The cerebral cells constantly are under the urge of stimuli, constantly are storing up material for thought. This material carelessly acquired, loosely stored up, is ever-ready to respond to stimuli that may later, call it into consciousness. And the response, unless under the direct control of the individual follows the line of least resistance. It may be connected and logical, or it may be disconnected and bizarre.

And so we see in "the Unconscious" merely reflex action. Here we have action, reaction and interaction but it is action, reaction and interaction outside of the consciousness of the individual. He does not control any portion of the process, neither the reception of the stimulus, nor the distribution of the secondary stimuli set in action by it. We must of necessity designate this as reflex action, for how else may it be designated? It is reflex action, reflex consciousness, cellular consciousness. It is not unconscious action for there can be no such thing, no more than there can be subconscious action. Consciousness is always present somewhere, wherever there is action, and wherever there is action and consciousness there is mind. Mind constantly seeks expression. This is the method of the mechanism of the mind, and the manifestation can be nothing else, no matter how bizarre that manifestation may appear. And so we have consciousness even in reflex action, and there the terms "unconscious," and "subconscious" may not apply.

But now there is the brain to be considered, the brain the storehouse of the mind. The mind, active, draws upon this storehouse in order to continue its activity; but the mind cannot use all the material at one time. While portions of the brain are responding to the demands of the mind other portions are quiescent. Some portions are in consciousness, others are not in consciousness, they are nonconscious. The non-

conscious areas await the opportunity to serve the individual. They await his summons, but sometimes they mistake *a* summons for *his* summons. *They will respond to any summons coming in over the accustomed route.* But this must not surprise us. The cell itself has no means of knowing who sent the summons. Its function is to receive the summons and to reply. If it receives a summons it must reply and it replies, often, through sheer force of habit. The touch-and-go reflex takes the reply out of the control of the individual. The cell receives the stimulus and acts upon it; but the individual may not be conscious of the reaction at the moment. At the end of the reaction he may sense it, but while it is going on, he does not. In this the reaction is entirely reflex. This might be called subconscious, having reference to the individual, but it is not subconscious in that it is action associated with consciousness. The reaction is outside of the individual's immediate recognition, but it is not outside of his mind. The individual is not always aware of the action of his own mind, but, even though he may not be aware of it, *it is his mind that is acting.* His mind is exercising its inherent quality, but it is doing so automatically. The brain cells are functioning as they have functioned through long ages. They are responding to the constant stimuli all about them; they are never at rest. This may be subconscious as to the individual, but it is not subconscious mind, for there can be no such thing as *subconscious* mind. There may be a fractional consciousness[1] a consciousness only partially developed but it still remains consciousness; it is mind seeking expression, however crude the expression. *Mind is Consciousness.* Any cell that responds to a stimulus must be conscious during that response. It could not recognize the nature of the stimulus and return a fitting response, unless it were conscious. But the consciousness is only momentary; it has neither past nor future, only present. It is reflex. After the cell has completed the reaction it has no memory of what it has done; and it does not undertake the reaction with any idea as to the future effect of that reaction. It merely is responding to a stimulus.

It is, then, not so difficult to understand the disorders of mind. There is behind every disorder, in fact, behind every manifestation of mind, the brain, the *nonconscious.* Reflex consciousness and conscious consciousness reacting with the brain give the varied manifestations. Consciousness takes from the nonconscious, the result depending upon whether

[1] "The Development of Consciousness" by the writer in the New York Medical Journal for January 11th and 18th, 1919.

consciousness is reflex or conscious. If it be conscious, well and good; if it be reflex it may be neither well nor good. There may be, too, a combination of the two; the manifestation may be mixed. Reflex consciousness and conscious consciousness using the same machinery often clash and we have then "confusion worse confounded." But through it all there is not to be perceived anything of the nature of the unconscious at work. The unconscious cannot work, cannot act; there is only consciousness in action. The cerebral cell responding to stimuli responds *in consciousness*. How else can it respond?

THE PSYCHOLOGIC ASPECT OF FREE-ASSOCIATION

By Theodore Schroeder

Psychoanalytic therapeutics with its technique for dream interpretation consists largely in the use of "free associations," with dream factors. This and the resultant theory of a "latent content" for dreams are often the points of attack for those who still have a sceptical attitude toward psychoanalysis. Such critics assert that the free-association products and their predominant sexual content are the product of suggestions from the analyst. These suggestions, together with the organic condition of the dreamer, are said to explain fully all the sexuality in the "manifest content" of the dream and in the erroneously alleged and non-existent latent content of dreams. For the psychanalyst such explanations seem utterly to miss the psychogenetic viewpoint. To justify the latter and to clarify the psychologic theory of dreams and of free associations is the purpose of this essay.

To eliminate the possibility of suggestion by a second party I must supply an illustration of free associations which developed when practically no possibility of such suggestions existed. Manifestly then these associations must be such as have come into consciousness when no second person was present, and when no psychoanalysis is in progress. This seems to limit me to some of my own personal associations, and their underlying experiences. Fortunately I recently had an exceptionally good experience for this purpose. For the sake of clearing up the situation, I will expose myself to the public with that frankness which is characteristic of the dealings of the patient with his psychanalyst. To this end I am going to tell the story of an unpremeditated and unexpected group of associated words, and their elaboration by further associations. Around this will come some discussion of psychologic theory.

An Undesigned Spontaneous Association

At the time of the occurrence about to be related, over three years had elapsed since I enjoyed professional associations with my psychanalyst. In the meantime I had seen

him but once. This was over a year before the occurrence now to be related. Then I saw him for only about an hour and had no conversation with him about my personal psychology. I call attention to this as showing just how improbable would be the assumption of any efficient immediate suggestion from him as a determinant of the precise character of the following associations and their antecedent underlying events.

Early one morning I was sitting in my library alone. I had before me an attempt to imitate my handwriting as revealed in my signature. The penmanship was that of a woman, my literary assistant. My fancy ran back to my adolescent period when I had indulged in some practice to insure a uniform and well rounded signature. I am now 54 years of age and at the time now in question I was not conscious of the femininity of the person whose "forgery" I was inspecting, or of any influence of that femininity upon my subsequent thought-associations. Neither was I, at the time of recording the associations presently to be mentioned, conscious that my own adolescent practice toward perfecting my signature was in part motivated by a desire to make myself more attractive in the estimation of girls with whom I might have correspondence.

As I recurred to my adolescent effort toward becoming a more proficient penman, I picked up a penholder and quite thoughtlessly I began to write. On recovering from my reverie, I found that I had written these words: *Abercrombie & Co. — Calcutta — Iroquoi — Magruder — Possum — Calcimine — Smart Set — Hangman — Nice — Lice — Porcelain.* A little later I was struck by the thought that without premeditation or any conscious design toward that end, I had produced an unusual group of words. What could have brought them together? My psychologic curiosity was stimulated to inquire what could have been the determinants of these apparently incongruous choices, and of their co-hering. Treating these words as symbols of things objective to my own mental state, no element of unification was apparent. Perhaps as symbols of my mental states it might be otherwise.

Obviously these words were all "free associations" with the penmanship of a woman, in whom I must confess having had an erotic interest.

In fancy I can already see some anti-analyst rise up to smite me. "Listen to a psychic-determinist talking about

'*free* association,'" I hear him shout in triumph. If this is a quarrel about the meaning or the most consistent use of words then the critic has the best of me. Such criticism may give him joy through a consciousness of dialectic superiority and will be very satisfying to some opponents of psychanalysts. However, such criticism will not give even the slightest aid to an understanding of my meaning or to psychologic theory. One who was concerned about understanding the concept that I was trying to describe, more than about a quarrel concerning my consistency or choice of words, would act otherwise. Such a person might discover that I did not really mean that these associations were "free" in the sense of the free-will theory. On the contrary, I was and am predisposed to believe that there is an absolute but then yet unknown determinism in every factor of that association. What a determinist means by "free association" is a freedom from *conscious* restraint against giving expression to whatever comes into one's consciousness. He does not mean that either in its coming or in its essence there is anything free from the determinism of antecedent experiences nor that the desire to express or withhold it is free from the influence of our own psychologic past.

Psychic Factor of Unification

Relying on my past experience in such matters I might tentatively say that it is impossible for the woman's penmanship to stimulate the writing of this group of seemingly incoherent words, unless there was some element of unification for them within my own psyche, produced there by my own past. Indeed it is doubtful if it is worth while to attempt any scientific investigation of the psyche except for the discovery of more of the factors in an absolute determinism, or on the assumption that there is such a completely determined causation, working under some approximation to uniform behavior, depending upon how near we can approach an exact duplication of the conditioning factors.

Those who have an aversion to the uncovering of their own past, as that may be still operating in their own subconscious impulses, will be content with pointing to the organic conditions and the objective contributions to the final result, and will insist that these supply an adequate explanation. For them, obviously the woman's handwriting and the fact that I am a male are the responsible stimulant for these words. These with their necessary ramifications are the only causative factors that need to be considered. "We have found

that much of what has been attributed to *mythical* psychological 'complexes' can be definitely referred to specific organic conditions," writes Prof. C. Rahn, (Psychol. Bulletin, Feb. 1918, v. 15, p. 40.) For many of us this seems to miss the more essential psychologic viewpoint. When I suggest that the woman's handwriting was only the objective stimulus to some complexes of mine, and that the actual words which came forth were the products of a choice subconsciously determined by my own past, they may protest as does another professional critic. He says: "This seems to turn things topsy turvy, since what A arouses is conceived as arousing A." (Prof. Woodworth in: Journal of Abnormal Psychology. 12 [No. 3]; Aug. 1917.)

So long as you see the woman's penmanship or my organic conditions as the only causal factors, you are dealing only with the more obvious and least psychologic aspects of the problem. Psychologically, the more important question is that which inquires: From my past life, what is the personal psychic contribution toward determining the choice of *these special words* in preference to all other words, as my response to the stimulus of this woman's handwriting? Probably no other person on earth would have made just that same response to this same stimulus. Try it out experimentally if you have doubts. The discovery of the personal contribution from my own past life, as a determinant of *the precise character of this response* to this objective stimulus, that is the special function of the psychanalyst. Let us restate this psychologic problem as it may be presented by the psyche of some of the more vehement opponents of psychanalysis. The question then would be this: What is there in their own past lives and submerged complexes which operates as their subjective contribution to the resultant emotional aversion which some of them manifest, when stimulated by psychoanalytic theory? Elsewhere I have illustrated a method for unravelling this mystery by the use of their own literary output.[1]

Those who have not faced themselves and their own past, as that is operating subconsciously in the present, or who have not faced all that in such a manner as is usual in the psychoanalytic process, must of necessity feel with some critics that: "It is contrary to the general fact that shifts of thought are the rule, so that you tend to get quite away from

[1] See: A Psychologic Study of Judicial Opinion. *California Law Review*, Jan. 1918. For a different technique see: Freud's Leonardo da Vinci. Also Kemp's, Charles Darwin, *Psychoanalytic Review*, Apr. 1918; vol. 5, pp. 151-192.

the context of your starting point in the course of a reverie or other free-association process. . . . and it is contrary to the fact of multiple possibilities of associative reaction. Thought may jump hither or yon. It is far removed from the fixed linkage of stimulus and a response characteristic of the simple reflexes . . . shifting of the topic is as characteristic of uncontrolled thinking as *to make it utterly impossible* to accept the fundamental proposition on which the Freudian conclusion as to the latent content of dreams etc., are supposed to be based." (Prof. Woodworth in Journal of Abnormal Psychology. 12 [No. 3]; 178; Aug. 1917.)

For many this seems to be very obviously true. Perhaps it would seem less true if they had ever given an aggressive co-operation in submitting themselves to a thorough psychanalysis with the aid of an efficient expert. This might give an enlarged view of psychic causation, of mental mechanisms and of psychic determinism, such as would make it impossible for such a person ever again to pen such a statement. There was a time when the quoted words might have appealed to me. Since I have come to a better understanding of the behavior of my own psyche, such statements have lost their appeal. This is so because now I have learned to look within *as well as without* for a more complete explanation of the shifts of thought and for the linkage of responses to stimulus.

Returning to the Associations

Now I must return to my word-associations, and show why it seems to me that an application to the psyche ot a more penetrating psychologic insight used for the discovery of the linkage between stimulus and response, would make some critics less ready to deny the existence of a very fixed linkage of thoughts which are not consciously controlled. Now let me return to my associations.

Upon discovering that I had put forth an unusual combination of words, I first tried to find some objective connection between the words themselves, or between the objective things which these words usually symbolize. I failed in this undertaking. Had I not previously received some psychoanalytic discipline, I might have stopped my investigation. If I had never been tempted to look for the thoughts and feelings *within me* which these words also symbolize, I would have neglected the more psychologic aspect of my problem. Had I failed to do that I might never have found the elements of unification in these word-associations. Then I would doubtless have agreed with the critics above quoted.

Under such neglect on my part, I would never have discovered the latent psychologic import of the choices which were unconsciously, yet effectively, expressed in these word-associations. To find the subjective import and elements of unification of my unique group of words, I resorted to further separate associations with each word. Relying upon past experience, I assumed that each of these words must be the centre of a complex, united by some common underlying feeling tone, with the other complexes symbolized by each of the remaining words, and by the stimulus which brought them into being.

The essays above quoted from had not been published then. Without that stimulus and as a mere matter of curiosity wholly unconnected with the present purposes or uses, I was prompted to concentrate my attention on separate words of this collection to see what images from my past would present themselves. I now remember that I did not follow the order in which the words were written and I do not now recall what order I did follow. But I can reproduce the pictures out of past experience which came into my mind, as the further association to each word in the group. These memories will now be reported. They will exhibit a subjective element of unification and of causation for the particular choices expressed in the above group of words, and I believe they will also illuminate the behavior of the psyche, so as to contribute something to the clarification of psychologic theory.

Abercrombie & Co. This first suggested a civil engineer named Abercrombie under whom I worked in New Mexico, when 18 or 19 years of age. Out of my brief association with him, only one incident concerning him sticks in my memory. We were living in movable camps, making a preliminary survey for a railroad. On returning to camp one evening after a whole day out, far in advance of the party, he told us of his experience with a Mexican woman. He went to her hut in quest of something to eat. He could not speak Spanish nor could she speak English. His first gestures being inefficient, he exhibited a coin to help the rubbing of his abdomen as a suggestion that he wanted to buy food. The woman promptly lay down on the floor and pulled up her dress. We boys in the party thought this a particularly good joke on the old man because of his extreme puritanism, for which the rest of us had little use. In my own case this was of course contemporaneous with the anxiety about the perfection of my penmanship and with the stormy period of pubescent readjustment. But whence came the "& Co.," at-

tached to the name. This did not develop until latet. and I will leave its explanation until it is logically connected with another event.

Macedonia. In this connection there came to my mind a priest of some far Eastern cult whom I saw in Wisconsin, at about my 18th year. He lived in a tent, wore queer clothes, travelled from place to place without money or much apparent forethought and preached strange doctrines. At this time I have no independent memory that he came from Macedonia. But in the light of other experiences the association is some evidence that I then believed that he came from Macedonia.

His doctrines were of great interest to me then, although now I cannot remember what they were. At that time heretical doctrines had a great affect-value for me because my mother's protestantism had cost her ostracism from her catholic family. Furthermore my attachment to my mother I know had as early as my fifth year acquired an integration of sexual curiosity. That was my pre-adolescent sexuality, a forerunner of more conscious and more mature interest, incestuous in its real nature. This is one of the common facts of life that opponents of psychanalysis like to denounce very vigorously so they may better remain unconscious of it as having operated in themselves. By a familiar process of transference my infantile erotic interest in my mother attached itself to her heresy and later with enlarged understanding it expanded to a sympathy with heresy in general. Some other time I may portray the mechanism of the development of that influence through the years. Now it is enough to connect it with memory fixation in relation to the strange priest.

Calcutta. That is the present address of a woman whom I know. A few years ago she lived in a tent near my summer home. That was before I had submitted myself to an expert psychanalyst. Then I had only read some analytic literature. She was a literary worker and I tried to give her some needed help in unravelling some of her psychologic perplexities. We talked freely of her sexual life and I had entertained some sexual phantasies toward her. Also she had some remote resemblance to my mother in her outward appearance.

Now I come back to Abercrombie & Co. In this connection there came to me the thought that Abercrombie & Co., was a firm that dealt in sporting goods. I tried to verify this in a New York telephone book, and I found a firm of that

name who are tent makers. Then I remembered that some years before knowing the Calcutta woman I had purchased a water proof cover from this firm. This cover I intended to use for outdoor sleeping on the porch of a small building on the grounds where I spend my summers, which building is known as "The Wigwam." I now remembered also that on my suggestion the Calcutta woman had sent to this same dealer for some canvas and tenting materials. Here again we have a sexual factor giving affect value to accompanying objectives and furnishing an element of unification between objectives that are not usually connected with one another. Now also we can see the similarity of my emotional tones associated with Abercrombie the civil engineer of my Mexican days, and Abercrombie & Co. the tent maker.

Iroquoi. Here-from I secured some reminiscences of frontier work while I lived in tents in Dakota Territory, Iowa and Idaho. In each of these places I saw some Indians. The dominating picture relates to an experience in Idaho among the Black-foot Indians. This was about my twentieth year when the pubescent turmoil was at its height. I had seen a good looking young squaw around one of the wigwams and I became conscious of a desire to copulate with her. Here we see again the erotic factor furnishing a closer subjective element of unification between objectives that had some similarities of their own.

Smart Set. First I thought of the magazine by that name then of the "fast set" in "good society" to whom I feel that it caters. This again brought me to my student days. When not engaged in railroad surveying or in lonely hunting trips of hundreds of miles on horseback, nor in the joys of being a box car tramp, I would return to the University. My wanderlust and the wilds had brought me no satisfactory sex mate. So I came back to the University. Here I worked out the other side of my conflict, by trying to be a "dude;" by trying to be of the "smart set" as I imagined them. At times I fancied that I had almost broken into the "swell, fast set." Here was the antithesis of the box car tramp in phantasy and conduct. From the Wigwam I turned to the "palace" of the smart set, but still looking only half consciously for the ideal and safe squaw. At that time I would probably have denied all this quite vehemently, if accused of it. Had any one then told me that by psychoanalytic methods my lie could be detected I would doubtless have found an argument to prove to my satisfaction that there was little truth or virtue in psychanalysis or its theories. Organic conditions within

me, and the immediate objective stimulus would be accepted as an adequate explanation because I was unwilling to admit that these had once produced conscious erotic phantasies, and unwilling to admit that these are still working.

Hangman. This reminded me of a lawful hanging that I witnessed while still a University student. My nearest relationship to the "smart set" came through my joining a college fraternity. During a little "frat blowout" in Chicago, I met a member of my college fraternity who was employed in the office of the sheriff of Cook County. It was through him that I received a pass to witness the hanging. Here again we see the erotic interests of adolescence furnishing a subjective element of unification connecting words possessing no objective connection in themselves.

Nice Lice. This brought to mind a camp of railroad graders on the south shore of Lake Superior. I was again employed in the engineering department. The bunk houses of the laborers were very lousy. It was reported that at times a man would put his hand within his shirt, scrape as many lice as possible from his body and offer to bet that he had pulled out more than 50 lice on his hand. The mess-hall where these men were fed had among its assistants a daughter of the proprietor. This young woman was about as fine a specimen of physical womanhood as I had then seen. Of course, I entertained erotic phantasies about her. I thought of her as an extremely handsome (nice) female in a very lousy environment.

Porcelain. This brings to my mind some porcelain in the house where resides the woman whose penmanship initiated this chain of associations and who also is physically a handsome woman. I have already said that I have entertained an erotic interest in her. Here also we get a suggestion of the conflict between the love of freedom that impelled me to the grader's camp, where only tin dishes were used and the china associated with the social life of the smart set.

Magruder. This word bring me no associations. From the viewpoint of understanding my own psychology it therefore should be accounted as the most important word in the group. I say this because of the possibility that it is a distorted symbol for some experience that was at the time more shameful than the rest, and on that account may have been buried more deep among the forgotten unpleasantnesses of the past.

Possum. This takes me back to college days when with other students I frequently sang a possum "coon song," at

midnight and around the girls dormitory. Also with the social life of my college fraternity.—All this again brings out the love of snobbery, which was the other aspect of the conflict which also made the young squaw and the waitress of a grader's camp seem attractive.

Calcimine. This reminds me of a room in the dwelling place of the woman whose handwriting stimulated this series of associations. The room was streakedly calcimined by an unskilled workman, and it was a sleeping room. Again we have the obvious erotic emotional tone as an associate.

The Psychoanalytic Viewpoint

Now let us ponder a little over this group of associations. Obviously if I had looked only to the import of these words as they are defined in a dictionary, and as they symbolized things objective to myself, then everything said by the opponents of psychoanalysis would appear to be perfectly true. All these criticisms may possibly be equally true if you take into account the import of the original word-associations treated as symbols of something in the subjectivity of every person on earth except myself. But the whole situation changes as soon as we contemplate their possible significance as symbols of something of emotional value in my own psyche, the product of my own past life. This includes my concepts and feelings as being themselves symbolic of objective things. Now the words I wrote upon the paper become symbols of my subjective symbolization of objectives. From this viewpoint we are dealing with the more intimate aspects of psychology. If now you think of these words as symbolizing my subjective symbolization of the objective realities then they have a new and special significance (a latent content if you please) which is my exclusive property, dependent upon my own past experience in association with them. These experiences and their effect upon my psyche are necessarily unlike the experiences and effects within every other person who may be tempted to use these same word symbols. It is this personal meaning and the peculiarly personal valuation of the words that must be sought for if we are seeking to understand the psychoanalytic theory.

For the psychanalysts, no one has an adequate grasp of the psychologic viewpoint who sees such associations only according to the acquired meaning of words as symbolizing something objective to those who use them. The psychoanalytic viewpoint requires us to consider these words, all

words in fact, as symbols of symbols; that is to say, as symbolizing the subjective symbolization of related objectives. So we find their genetic meaning in the past experience as explanatory of the present choice of those who use them.

It is because the critics of psychanalysis have not generally acquired the habit of seeing psychoanalytic work and theories from this more purely psychologic viewpoint that they so often seem to the psychanalyst to be discussing something quite irrelevant to that which he wishes to debate. The psyche of each observer establishes a point of contact, a relational existence between the stimulus in the environment of the present and the subjective record of the environment of the past. The psychanalyst is primarily concerned with the latter aspects of this related existence. His critics seem mostly concerned with the other aspect of this same related existence. In so far as this is true they are obviously not discussing the same data. In one sense it may be said that the psychanalyst is essentially a behaviorist. But here again the same possibilities of controversy exist; the psychanalyst is not very much concerned with the outward and visible aspects of behavior, except as symptoms. His task is to understand the more subjective aspects of behavior, especially in relation to their origin in the past, and the mental and emotional mechanisms by which that past becomes an efficient element toward determining the present reactions to stimuli.

The Dream's Latent Content

Let us now suppose that instead of writing these associated words I had immediately gone to sleep under the influence of the handwriting stimulus. From some considerable study of my own dream formation as well as those of others, it seems quite possible that instead of these associated words as visual images of penmanship there might (and probably would) have appeared a succession of phantasies something like this: A tent maker is seen in the streets of Calcutta and there comes along an Iroquoi Indian followed by a possum. They stop in front of a strangely calcimated house of the aristocracy; or was it a house of prostitution? A handsome woman comes out and on a beautiful porcelain plate passed around some lice. Assuming now that the same impulses which actually produced the written words had instead thereof produced some such dream phantasy, then the association process would have led to the same group of past experiences as were brought out by the associated words.

Under these circumstances surely it would not be too great an abuse of words, to say that the separate, fleeting and incomplete parts of the dream phantasy each symbolized *for the dreamer* the more complete experience of the past, just as did the written words. From this past experience the present stimulus has brought into consciousness only such a fragment as perhaps was the focal point in the past emotional experience with the objective associate, or that best served the purpose of disguise for unpleasant factors, all these being connected in the past by feeling tones similar to those now stirred into subconscious or conscious activity by the stimulus of the present.

Again: The remainder of the past experience, not presented in the dream phantasy or necessarily implied in the associated word, can later be brought into the surface of consciousness by the further concentration of attention on that which is already present in consciousness. The result is expressed as "free associations." So more and more of the past is brought into the consciousness of the present. In its reductive aspects psychoanalysis consists largely in an efficient and elaborate technique for accomplishing that end, and exhibiting the influence of that past in the conduct of the present. It does not seem to me that it is a great abuse of language to describe this supplemental material of the free association process as the "latent content" of the dream symbolism.

Of course the professional psychanalyst will see in this narrative of mine much more than the mere words reveal. In all of my associations I have again evinced a choice just as certainly predetermined by the remainder of my past as was the choice of the original group of words predetermined. By a sufficient succession of free associations, applied in virtue of the similar psychologic imperatives and mechanisms, the whole of my past life could be revealed.

The Critics of Psychanalysis

In thus describing one incident in the behavior of my own psyche there has been presented a partial picture of the processes of reverie and dream formation. This is only an illustration, not a demonstration of the theory within which it might receive general formulation. The professional analyst after acquiring a close view of very, very many such psychic mechanisms, the processes of dream formation, their symbolisms, latent content and subsequent elaboration, is perhaps better qualified than most of his critics, to formulate

a theory of intellectual and emotional mechanisms, even though he cannot secure the space for reproducing all the materials acquired from scores of patients. Of course it is quite true that much of the misunderstanding about psychanalysis is due to the analysts' shortcomings as controversialists. This in turn is partly due to the fact that their literary output is mainly prepared to appeal to their own group and not addressed primarily or adequately to the mental attitude of emotional opponents. For reasons obvious to the psychoanalyst his literary output does not easily adapt itself to overcoming the emotional resistances of critics.

Right here, it seems to me, is the place for making another criticism upon the more emotional of the critics of psychanalysts. It is a complaint against the critic's unscientific attitude toward the whole subject; so obviously unscientific that the analyst would be perplexed to explain it had he not found scores of similar resistances among his patients. Let me try to state this with the special purpose of making it plain to those who are most in need of being corrected.

We may imagine one coming to a chemist and announcing a very new process for transmuting copper into gold. The innovator with many competent chemists as sponsors challenges investigation. What would an open-minded but doubting chemist with the best scientific spirit do by way of investigating the new theory and method? Obviously he would bring a piece of pure copper and other required chemicals of his own selection, and with the greatest care possible he would follow all the directions given and then from an examination of the product of that experiment he would determine whether gold had been made from copper.

The most vehement and most facetious critics of the psychoanalysts are those who have refused to go to the laboratory and have refused to bring their own piece of copper for the purpose of duplicating the psychoanalytic experiment. In this latter case the copper is his own psyche, the necessary chemicals his own past experience. The critics of psychoanalysis are not meeting the challenge to bring their own psyche and give an aggressive coöperation for its analysis. When they have done this and so have made the best possible exertion to see if the psychic energy behaves as reported by the psychanalyst, then he is qualified as a critic. Strange to say, none of those who fundamentally disagree with the psychanalysts have ever submitted to the test of an adequate expert analysis as a qualification for criticism.

Those who have acquired a close-range view of the ma-

chinery of their own psyche are thereby better qualified for the understanding of the psychic mechanisms operating in others. As between psychanalysts it is a general truth that relative efficiency depends mainly upon the thoroughness of the analysis to which they have first submitted themselves, and the quantity, diversity, and complexity of cultural material which has been integrated with their psychologic intelligence. In so far as they have not acquired an objective view of the behavior of their own psyche, to the same extent will they be unable to achieve a clear understanding of their analytic subjects. In such circumstances and to that extent will they read their own misunderstandings of themselves into others. Thus many become failures as professional analysts. This is a commonplace among psychanalysts. The critics of psychanalysis lack a similar understanding of this same limitation operating in themselves.

The progressive clarification and formulation of psychologic theory on the basis of the revelation made to the psychanalyst is still in the making. Here there is much room for discussion and criticism by those who can give sympathetic understanding, and who will remember that for psychologic disputes, words should be usually understood as the symbolizing of our mental symbols of objective realities, and not as direct and immediate symbols of those objectives. Critics who combine these two qualifications will be of great help to the advancement of scientific knowledge from the viewpoint of the psychanalyst. Even psychanalysts at times fail to keep this distinction in consciousness. To the extent that this is so they classify themselves among the relatively inefficient discussers of psychologic theories. With the critics of psychanalysis this more intimate psychologic viewpoint is quite commonly overlooked. When that is the case the critic is not debating that in which the psychanalyst is most interested, and the discussion is irrelevant.

THE FREUDIAN DOCTRINE OF LAPSES AND ITS FAILINGS

By A. A. Roback, Harvard University.

Whether Freud's interpretation of lapses constitutes an integral part of his system or not, of one thing we are certain, and that is the growing importance of that phase of psychology as throwing light on every day occurrences, which, insignificant in themselves, are often the vehicles of portentous events.

We seldom notice a slip unless it assumes the proportions of a blunder. Sometimes we are prone to attach an ominous significance to such an occurrence, which fact might account for the halo of mystery surrounding the subject in semi-intellectual spheres. Aside from that, however, there is something fascinating about a situation where one acts, as it were, in spite of himself. It is our intention to say one thing, yet we say another. We purpose writing a certain word or phrase; our fingers, however, execute a set of movements other than those intended. What is at the root of this mischief.

The current phrase "forcing the hand" expresses the feeling rampant among the common people—as if the act were of extraneous origin. It is represented as the precursor of either a fortunate or an unfortunate event. Thus it takes the form of a symbol announcing the advent of the happening proper.

It is now evident that here we touch on Freudian territory. No one, of course, would think of imputing to the Freudian School the primitive view just referred to. Freud would be the last man to trace a slip to extraneous causes or to regard it as indicative of a future good or evil. But it is *symbolism* nevertheless which connects his doctrine with the crude belief of the man in the street. The lapse according to Freud and his followers, is symbolic of the *hidden motives* at work in the mind of the person guilty of the slip. The slip might then be said to manifest itself as an interference between the open intention and the underlying motive, the former being misplaced by the latter.

Curious as it may seem, Freud's doctrine of lapses has been rather immune from the darts of his many critics, though of all his doctrines, it is the only one which is amenable to experi-

mental investigation. Aside from the positive arguments that may be invoked in criticism of his conclusions, there is much in his account that is impugnable on the strength of common sense.

If asked to sum up in a word the failing of the Freudian interpretation of lapses, one might say it is *misrepresentation*. In most cases emphasis is laid on irrelevant details, while essential facts are ignored. Freud takes a dash in the wrong direction, to begin with, and leaves no room for simple explanation.

The writer does not wish to convey the impression that he is belittling Freud's valuable service in other fields. Quite the contrary, he believes that Freud has opened new vistas in psychology,[1] but it happens that in his eagerness to subsume every human act under his all-embracing doctrine, he is blinded to a number of simple facts. Some of the interpretations in the "Psychopathology of Everyday Life" fairly make us gasp. Not only is there not the slightest plausibility in the tentative explanation, but every bit of *vraisemblance* is occasionally lacking, unless we assume that his patients belong to the class of people characterized by the heroes of Dostoëvsky's and more particularly Artzibashev's novels. And if so, the question arises as to what degree of trustworthiness can be attached to them. Can we, for a moment, think that a woman patient who was "ashamed" of herself for having made a slip of the tongue when asked how her uncle was, by saying, "I don't know, I only see him now *in flagranti*," when what she meant to say was, of course, *en passant*—can we imagine that she would try to vindicate herself the next day of the possible imputation that she was not an educated person by reproducing "a reminiscence..... in which being caught *in flagranti* played the principal part"?[2] And the conclusion is that "the mistake of the previous day had therefore anticipated the recollection, which at that time had not yet become conscious."

That is not all. The critical reader will often have reason to suspect that certain characteristics and traits are occasionally read into the poor patients *ex post facto*. A young woman "who is the domineering spirit in her home"[3] is so naturally because her speech lapse fits in with such a characterization.

[1] A. A. Roback. *Menorah Journal*, vol. v., 1919, pp. 24-32.

[2] Freud: Psychopathology of Everyday Life, p. 84. (Brill's translation). The passage is poorly translated. It should read " . . . in which the fact of being caught *in flagranti* figured most conspicuously."

[3] Freud (Brill) *loc. cit.* p. 91

Another woman "who was especially weighed down by the financial burden of her treatment" is probably credited with this circumstance by Brill,[4] just because she actually said, "Please do not give me *big bills*", when she meant to say "big pills." Brill attributes the lapse to the patient's circumstances. We prefer to take the simpler course and say that the mistake was due to a repetitionary assimilation in the direction of the easier sound, as it requires less effort to articulate a "b" than it does a "p."

The Freudians have not learned the truth of Occam's principle "*entia non sunt multiplicanda praeter necessitatem.*" They rather seem to put a premium on the introduction of as many factors as possible—and to no purpose; for, entertaining as the stories and anecdotes are, some of them self-incriminatory,[5] they *do not tell us anything about the mechanism of the lapse.* Has it ever occurred to Freud and his own, that in most instances cited where positive and negative concepts been unintentionally interchanged, the fact *is that in practically every case the lapse is brought about by the elimination of the prefix un or in?* The reason for this is that in the excitement of speaking or writing, we fasten our hold, often anticipatorily, on the stem of the word and not on the prefixes, which are separable and which in themselves are not significant. The important part is the "fortunate" and not the "un,"[6] the "selfish" and not the "un,"[7] the "responsible" and not the "ir."[8] Stekel and Freud may make much ado about a slip where "in eigennützigster weise" was written instead of "in *un*eigennützigster weise,"[9] but rather than say that the author of the slip was actually accusing himself of selfishness, why not look into the formation of the word misspelled? Might we not expect, in the case of a clumsy compound, drawn out in the superlative form, at least the lopping off of the negative particle, especially when there are already two n's in the stems of the word? It is really a matter of omission, and as abundantly proven, it is the particles and less important words that are omitted in haste, excitement, embarrassment and so on. The inexperienced public speaker will find that the substantives come to him readily but the prepo-

[4] Freud *loc. cit.* p. 103.

[5] The self-incriminatory character of these allusions is so striking that they might be regarded as manifestations of a sublimated exhibitionism.

[6] For the references in this connection *cf. loc. cit.* p. 425.

[7] *loc. cit.* p. 126.

[8] *loc. cit.* p. 106.

[9] Freud: Psychopathologie des Alltagslebens, 2nd German ed. p. 55.

sitions are rather tardy. Stoll, too, has reported[10] that in the copying of the meaningful texts, the words most omitted were those that were of secondary importance. From our own results, we can extend this observation to cover letters, for as compared with the large consonants (letters like j, k, p. f, etc.) the vowels, semi-vowels, and small consonants were slurred.

The upshot of this criticism is that Freud and his associated are not warranted in attaching a hidden meaning to such lapses on the strength of uncritical and one-sided observations. If the writer intended to say that many of the Freudian arguments are inane, and his interpretations inept, and wrote instead of inane and inept, the stem-components "ane" and "ept," Freud might have had some slight ground, on his principle, for assuming that at bottom we are in sympathy with all his doctrines. Our protest would not be so loud in such a case against the assumption, *because the words "ane" and "ept" are not used without the negative particle.* Hence it would mean the breaking up of a graphic habit—a disruption which requires explanation. This concession, on our part, however, would not hold if the word intended were "inapt," for "apt" is a common word in every-day use; and so the lapse would originate in accordance with our previous explanations. The only case of a *lapsus calami* where the larger negative form "unhappy" was substituted for the intended word "happy" is given by Ernest Jones[11] who was told by a lady that an "old friend in writing to her closed the letter with the curious sentence, 'I hope you are well and unhappy.'" According to Jones, "the slip of the pen was evidently determined by his dislike at the thought of her being happy with some one else" as he had entertained hope of marrying her himself. Even this solitary case pointing apparently to the operation of a wish complex may easily be explained by supposing that the slip was occasioned by the conflict not of *two wishes*—the one censored and the other primitive—but rather by the interference of a *wish* and a *fact,* the mental attitude corresponding to the expression. "I hope you are well and happy, though I am unhappy." In other words, the antithesis is not between two states in the future, but between the future and the present. There is no need of positing even here a hidden wish mechanism. Most likely, the writer was actually thinking of his unhappy condition at the time he was concluding his letter,

[10] J. Stoll: Zur Psychologie der Schreibfehler, Fortschritte der psychologie, etc., vol. II, 1913-14, pp. 22-48.

[11] E. Jones: Papers on Psycho-Analysis: p. 63.

and the various imaginal elements that went to make up his mental attitude, were probably synthetized by the word "unhappy."

There are, nevertheless, two or three accounts of a *lapsus linguae* in Freud's (Brill) *Psychopathology of Everyday Life* that we can become reconciled to, but only after definite qualification.

Brill relates of an admirer of Roosevelt who remarked to his host on the occasion of an evening dance,

"You may say what you please about Teddy, but there is one thing he can always be relied upon; he always gives you a square *m*eal."[12]

What he was about to say was a "square *d*eal," but the guests had reason to be disappointed in the hospitality of the host, and this embarrassing slip gave away the situation.

We have no difficulty here in accepting Brill's implication, provided it is admitted, on the other side, that the speaker had been *thinking* of the word "meal" that evening and probably the idea came up several times and, that too, suffused with emotion, as in the case of every hungry man. Hence there was with him what we should call a "predisposed attitude." Now the fact that square is used with both meal and deal, and that the two words are so similar in sound much helped the "predisposed attitude" to bring about the association and finally the unfortunate slip.

In the same way also is it understandable that a person who entertains high ideas of his importance would sometimes substitute an "I" when he is referring to other people, especially those he looks up to. Here we may say that a "prepotent attitude" (congenital) is at the root of such lapses.

Freudians will probably seize with great avidity on a passage in ex-ambassador Gerard's *My Four Years in Germany* as tending to corroborate their complex doctrine of lapses; and, for their benefit, the passage is reproduced here.

"Monday afternoon there was a Bierabend in the large hall of the Yacht Club at Kiel. The Emperor was to have presided at this dinner, but his place was taken by his brother, Prince Henry. Sir Edward Goschen, the British Ambassador, who was living on one of the British battleships, sat on his right and I sat on his left. During the evening a curious incident happened. The Prince and I were talking of the dangers of after-dinner speaking and what a dangerous sport it was. In the midst of our conversation someone whispered to the Prince and he rose to his feet, proposed the health of the visiting British Admiral and fleet and made a little speech. As he concluded, he said, addressing the officers of the British fleet. 'We are sorry you are going and we are sorry you came.'"[13]

[12] *loc. cit.*—— p. 102.

[13] James W. Gerard: *My Four Years in Germany*, p. 107

What we should insist upon, however, in opposition to Freud and his school is that emphasis be laid on the *actual association* in the speaker's or writer's mind between the word intended and the mis-expression. The association most frequently is, but need not necessarily be, verbal. It may be kinaesthetic, organic, and what not, but pregnant with meaning just the same. Thus we are not obliged to resort to unknown and hidden unconscious forces that are constantly distilling sexual and other complexes.

On the subject of writing mistakes, Freud's material is rather tenuous, this time not only in quality, but also in quantity.[14] There are really very few illustrations here of genuine writing lapses, as Freud does not take the trouble to separate out the memory lapses, and deals with several kinds of mistakes indiscriminately. Yet when we do come across a *lapsus calami* proper we find much ado made about it. He takes two pages to account for a slip of his that could be done to much better advantage in a few lines. Here is the instance. Wishing to draw some money from the bank, he glanced at his account, and finding that it was 4380 crowns, he decided to bring it down to the round sum of 4000 crowns. Upon making out the cheque, however, he noticed that he had written 438 instead of 380 crowns.[15] Common sense and experimental evidence[16] would lead us to believe that while his attention was occupied with something else, the determining tendency to substract 380 was in abeyance. Hence his action incorporated the elements of perception of the figures and the marginal idea of subtraction. Some figure was to go. Now in a number like 4380, what impresses one is the 438, not the cipher. The 438 is more like a unit. It has more significance, besides the 0 comes at the end, and in many of our computations, for instance, in division, we have occasion to lop off the cipher at the end. Under the determining tendency to subtract mentally, this mathematical stereotyped act reinstated itself and the 0 was disregarded making the number 438, which was promptly copied on the cheque.

In the opinion of the writer, this explains amply what has taken place psychologically, but Freud naturally must introduce a whole series of numbers and weave into his material

[14] A rather significant fact when we consider that Freud recognizes together with Wundt, there is a greater tendency to make slips of the pen than slips of the tongue (cf. his *Psychopatholgie des Alltagsleben*, 2nd. edition, p. 56.)

[15] *Loc. cit.* pp. 122-123

[16] *cf.* A. A. Roback: *The Interference of Will-Impulses, Psych. Rev. Monogr.* Supplements vol. xxv, 1918, p. 91ff.

several interesting, though irrelevant, stories to account for such a comparatively simple operation.

Playing with numbers is a pastime much relished by the Freudians. In the Chapter on Determinism we are treated to a veritable number-salad which does honor to the old cabalists and mystic commentators of the Pentateuch, for in *their* exegesis, they were guided at least by a system of hard and fast rules. The commentary called Ba'al ha-Turim by *Jacob ben Asher* of the 14th century, has much in common with Freud's number-juggling, and it may be said that many of the *Notarikin* and *Gematrioth*[17] as they are known, contain sex allusions calculated to illuminate certain episodes in the Bible. Sometimes it would even seem as if Freud's race-fellow of the fourteenth century has proven himself the more ingenious of the two in that respect.[18]

From Freud let us pass to some of his devotees and see what they have to offer in the way of solving the problem of language interferences.

When a student writes April 11, 1911, instead of April 22, 1911, what simpler and more plausible explanation may we resort to than the fact of motor anticipation? Jones[19] rightly observes that "the date he actually wrote was the 11th, was no doubt influenced by the presence of these integers at the end of 1911." If, however, such a simple explanation were allowed to stand, then what would become of the Psychopathology of everyday life, which must be fed on illustrations of this sort? Hence the narrator interposes with a "but." The explanation is discounted by the psychoanalyst's saying: "But it is to be noted even in this connection that his mistake consisting [sic] in writing them earlier than he should, i. e. in putting the date earlier." Surely this offers no cause for mystification. Anticipation is just as legitimate a mode of making a slip as repetition. But furthermore, if Professor Jones had only taken the trouble to investigate the material, he would have noticed that in 1911 there occur four downstrokes, furthermore, that the student had been writing, in dates, the number 22 as compared with 1911 in the ratio of 12: 365. Why, then, on the basis of graphic habituation, is

[17] *cf.* Jewish Encyclopedia under Notarikon and Gematria respectively.

[18] A. A. Roback: *Freudian Psychology and Jewish Commentators of the Bible.* Jewish Forum, Oct. 1918.

[19] E. Jones: Psycho-Analysis, p. 63.

it at all strange that the student should have put the date earlier?[20]

The hypothesis that Jones put forth is to the effect that the student was unprepared for a certain examination due in the first week of May and wished, therefore, to have more time. But if a wish were really the determining factor of the lapse, the substitution should have occurred not in the *date* of the month but with the *name* of the month, as we may well imagine that to "be through" with an examination is a more pious wish with a student than to have the ordeal merely put off.

Every one of Jones's illustrations might be analyzed without reference to any wish complex. Thus when intending to write "Dear Fred" he substituted Freud for Fred,[21] the explanation is quite clear and does not call for the supposition that Jones had entertained a hidden wish to be on intimate terms with Freud. Jones himself admits that the circumstances of the slip of the pen were extraordinarily favorable to its occurrence—the similarity in the names, the previous identification of the men, the occasion of the letter following so soon after the other one and so on. The remarkable thing about the admission is that Jones has not recognized the fact that while he had been writing the word "Freud" hundreds if not thousands of times, not to mention the countless occasions on which he had seen the name in print, thought about it, articulated it, etc.,—while the name Freud then was playing the part of the sun in a solar system, the salutation "dear Fred" was being used for the first time as it was addressed to an old friend "whom I had always called by his surname."

Another instance of stretching interpretations is afforded by the following account again taken from Jones's book: "A young lady was secretly engaged to a medical man, whom we will call Arthur X. She addressed a letter one day not to Dr. Arthur X, but to Dear Arthur X, thus expressing her desire to let all the world know of their relationship.[22]

It is strange that in the section on misprints which Jones had published originally in a journal, a slip had crept in which, though corrected afterwards, was not commented upon, in his *Psycho-Analysis*. On the part of a Freudian the missing of any opportunity to make a case for the favorite cult is almost

[20] It is very amusing in this connection that, while Jones seems so puzzled about the student "writing them (the integers 11) earlier than he should," what should he himself do (or perhaps it is the compositor's fault) but make the very same kind of a slip; for instead of saying "consisted in writing" he has it "*consisting* in writing."

[21] Jones: *Loc. cit.* p. 65.

[22] Jones: *Loc. cit.* p. 65.

symptomatic as a "Fehlhandlung" and would, in its turn, require an explanation. The slip referred to consists of the word "brochure" being mis-spelt as "broschure." The sentence in the original article reads: "In a broschure of mine that appeared as a German translation, a mistake was made of a less unfortunate kind."[23] Were we as uncharitable as most of the Freudians are in their evaluations of phenomena, we might be inclined to say that the intercalation of the s in "brochure" was due to the submerged feeling on the part of the author that the pamphlet or its substance was obscure, but in order to forestall any such ingenious interpretation, might we not suggest that the slip was made on account of the German setting produced by the circumstances described in the sentence quoted?

The following up of writing lapses by Freudians, who in their very comments make more slips thus affording the opportunity for subsequent writers of the same school to make additional observations incorporating ever new slips—this whole process is a veritable "comedy of errors." Let us give an illustration of this entertaining game.

In his papers on Psycho-Analysis, Jones had written that "distinctness of calligraphy is powerless to prevent such mistakes."[24] This statement occasions Holt to make the following observations: " 'Why *calligraphy*'? thought I, since of course calligraphy is necessarily distinct and Jones besides being a careful writer perfectly knows his classics. Of course he had unconsciously written 'calligraphy' instead of 'chirography,' because of the delicate boast which is thus conveyed, that his handwriting is always, even when indistinct 'beautiful.' To this extent his ego-complex had eluded his censor.. This was too good to lose, so at the bottom of the page I wrote in pencil, with reference to 'calligraphy' above, 'Should be 'chiro'—: Another case of *Verschreiben* [*lapsus calami*] *w.* odious cause.' And then the joke was on me. I had fully intended to write 'obvious' and was as astonished to see 'odious' as if another person had written it."[25]

Well, it was my good fortune to see Professor Holt's note in the original, and I may say that in his pencil note the word 'chiro' appears as 'cheiro,' so that in the very copying of the note, Professor Holt, who is punctiliously careful in such matters, had made a slip which was not corrected. But further-

[23] Jones: The Psychopathology of everyday Life: *"American Journal of Psychology,* vol. XXII, 1911, p. 503.
[24] E. Jones: Papers on Psycho-Analysis, p. 71.
[25] E. B. Holt: *The Freudian Wish,* p. 32.

more, the slip "odious" for "obvious" may easily be explained on another and simpler ground than the one furnished by Holt. Those who have access to that particular book, which is in the philosophical library of Harvard University, will find that the word 'should' in the note was written directly above the word 'obvious.' There are certain letters in words that stand out and catch the attention more strikingly than others, and the 'd' of 'should,' written as it was on that part of the page, is one of these letters. Professor Holt must have seen the d of 'should' just as he was beginning the o of obvious and copied it unintentionally; and being, as a rule, more critical than positive in his attitude, the word 'odious' has probably just about as many chances of occurring to his mind as the word obvious.

But the climax is yet to come. In spite of all the explanations and observations and inferences, Professor Jones has made no slip in the first place; for though he may "perfectly know his classics," he seems thoroughly grounded in his English, too, at least enough to be aware of the fact that there are good precedents for using "calligraphy" in the sense of mere handwriting, regardless of its aesthetic value. Had he written 'chirography' instead, he might have been accused of delicately boasting his perfect knowledge of the classics, even if he should not have laid himself open to the charge of pedantry.

Thus we see (1) that the supposed slip was a perfectly correct usage and should not have called forth the "odious" complex; (2) the substitution of the word 'odious' for 'obvious' is happily explained, and (3) the real lapse—that of writing 'chiro' for 'cheiro' as it appears in the original note—was not recorded.

A misprint in which the Austrian prince was referred to as a "*g*egenstiftender Mittler" instead of "*S*egenstiftender Mittler" occasions A. J. Storfer[26] to go into certain details bearing on the political relations between Austria and Hungary. If Storfer had only laid his history aside and examined the sentence more closely he would have seen that in the words "beruhigt den gegenstiftenden, etc.," the substituted "g" is an assimilation caused either by the perseveration of the "g" in *beruhigt* or by the anticipation of the "g" in the word *Segenstiftenden*, or both, the perseveration determining the anticipation.

[26] A. J. Storfer: *Zur Psychopathologie des Alltags Intern. Zeitschrift für ärztliche Psychoanalyse*, vol. II, 1914, p. 172.

In expressing his condolence to a young woman who had recently lost her husband, someone had employed the words:

> "Sie werden Trost finden, indem Sie
> sich völlig Ihren Kindern widwen."

This apparently is a case of repetitionary assimilation, partially determined by the consciousness, on the part of the speaker (or writer), that he was addressing a widow. Th. Reik's[27] interpretation of this perfectly harmless slip is that the suppressed idea alluded to consolation of a different sort, viz:

> "eine junge schöne WITWE wird
> bald neue Sexualfreuden geniessen."

Comment on this is unnecessary.

At times it looks as if the "complex" mechanism were to operate in a prophetic manner. The minor Freudians never allow themselves to stop before any extravagance in drawing their inferences. Thus the mere substitution of a D for a P in the word "Epithel," by a person whose acquaintance with an Edith was only superficial at the time of the slip but had afterwards become intimate, elicits the following explanation from the author: "Das Verschreiben ist also ein hübscher Beweis für den Durchbruch der unterbewussten Neigung zu einer zeit wo ich selbst eigentlich davon noch keine Ahnung hatte, und die gewählte Form des Diminutivums Characterisiert gleichzeitig die begleitenden gefühle."[28]

As usual, we are told everything that may have a remote connection with the origin of the lapse, but the sentence in which the word "Epithel" was to have occurred is not given. As to the dimunitive form(?) revealing the embryonic affections of the writer who was apparently not aware of them at the time they "accompanied" the slip, it is enough to say that the explanation is highly ingenuous and amusing. In all probability, the mistake was caused by either an anticipatory or perseverative assimilation, and *Edithel* was no more of a diminutive form than Epithel is. The assimilatory process may of course have been touched off because of a previous association which had gained a certain amount of vividness and, therefore, impressive force, but even so, we are not warranted in ascribing an oracular function to the subconscious mechanism presiding over slips of the pen.

An ante-bellum allusion to the great European conflict is

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[27] Th. Reik: *Fehlleistungen im Alltagsleben intern. Zeitschrift für ärtzliche Psychoanalyse,* vol. III, 1915, p. 44.

[28] Richard Wagner: Ein kleiner Beitrag zur Psychopathologie des Alltagsleben: *Zentralblatt für Psychoanalyse,* vol. I, 1911, p. 594

mentioned by Emil Simonson[29] who cites the report in a German newspaper of an interview given out by the then Russian minister of foreign affairs, Sazanov, to the *Novoye Veremya*. In this interview, Sazanov was quoted as discussing the friendly relations between the Triple Entente and the Dual Alliance (Zweibund), but as Italy, at the time, had been one of the members of the Alliance, to which the minister was referring, the quotation raised some comment in German political circles.

A diplomatic malapropism of this sort leads Simonson to remark that there was uncertainty somewhere as regards Italy's place in the Triple Alliance; and if the slip was made either actually by Sazanov or someone else on the Russian side, the likelihood is that it was caused by a feeling that Italy might be induced to part company with Austria and Germany, while if either the German correspondent in Russia or the newspaper in Germany was responsible for the lapse, the incident is to be explained by the fact that its author was distrustful of Italy's attitude and secretly harbored the belief that it would be better for Germany and Austria to remain a dual alliance.

Simonson's expectations, or rather knowledge of the situation turned out to be correct, but, interesting as this bit of information may be, it is irrelevant to the matter in hand unless we know definitely whether Sazanov has made a slip in the first place or whether it was his intention merely to stress the original core of the Alliance. Besides, in the report of an interview, so many different agencies are involved that we should only be groping in the dark for a foothold until we can trace at least the source of the lapse.

The Freudian complex distillery does not appear to be so productive in French as it is in German, yet we must not suppose that the French language is immune to misinterpretation, and, for the sake of variety, we might look into a couple of French versions of the Freudian story with a bit of Swiss local color.

R. Weber,[30] to begin with, gives illustrations of lapses occurring with the inmates of the institution with which he is connected. The *lapsus calami* which has been singled out as especially characteristic is one made by Weber himself when

[29] Emil Simonson: Ein interessanter Fall von "Versprechen" oder "Verschreiben." *Zentralblatt für Psychoanalyse.* Vol. II, 1912, p. 363.

[30] R. Weber: Petite Psychologie. *Archives Internationales de Neurologie,* 1912, reviewed in the *Zentralblatt für Psychoanalyse,* Vol. II, p. 536.

he wrote instead of *Peillard*—the name of a patient who was known for his wantonness—the word *Paillard* which is the vulgar expression in French for "rake." A pun of this sort is nothing but natural, but if Weber was not really aware of punning at the time he made the slip, we need only recall the fact that the word *paille* and its derivatives are so commonly used in French, while the word *peille* is not to be found in the dictionary—to understand the cause of the slip. The mistake, then, is only the expression of a motor habit.

Maeder[31] reports an elaborate bi-lingual lapse. In the first place, he could not, according to a dream of his, find the French word for "gauze," and so he asked for it in German, using the word *Mull,* but when he read over the dream he recorded, he found that instead of *Mull* he had written down *moule*—a word a friend of his family was in the habit of using as an equivalent for "blockhead."

The explanation unfolds a long series of associations which give us a roundabout clue of the origin of the particular lapse.

Much less satisfactory is an account of a lapse offered by H. Hellmuth.[32] *Levitico* (Wasser) had been substituted in a prescription for *Levico* (Wasser); and we are supposed to believe that at the root of this slip was the desire on the part of the physician to have his patient dress *quickly* and make room for another patient. Hence he had incorporated the French word "vite" (quick) into the mis-spelled German word (!). When it comes to fanciful explanations, the female Freudians are evidently not to be outdone by their male colleagues.

That it is not difficult to hit upon a Freudian interpretation can be seen from the following facts. Claparède[33] relates how one day he wanted to administer some laudanum to a patient, but asked his assistant to hand him the bottle of tincture of iodine. A careful introspective account follows as to how the mistake originated. He traces this lapse back to an associative dynamic which he would call mechanical, passive, or superficial in contradistinction to Freud's intentional, active or deep-seated dynamic. In another case, analogous to this he had said "bismuth" when it was his intention to say "magnesia." His explanation of this is similar. The visual

[31] A. Maeder: Psychologie de la Vie Quotidienne. *Archives de Psychologie,* vol. 7, 1908, p. 288.

[32] H. Hellmuth: Beiträge zum Kapitel "Verschreiben" und "Verlesen." *Zentrabllatt für Psychoanalyse,* Vol. II, 1912, p. 277.

[33] Ed. Claparède: De la représentation des personnes inconnues et des *lapsus linguae. Arch. de Psychologie,* vol. XIV, 1914 p. 503.

image of the two powders is the same (white) and governs two words with the predominance of the one of these visuo-verbal associations over the other. Are we to suppose, Claparède asks, alluding evidently to Freud, that I had a secret desire to constipate the patient who needed a purgative? And picking up a psycho-analytic journal, what do we find but that Ferenczi,[34] Freud's adjutant is inclined to accept Claparède's *reductio ad absurdum* question as the proper solution of the lapse.

Finally, we have still to analyze the case referred to by Menzenrath who repudiates the doctrine of Freud, and yet comes very close to adopting Freud's method and certainly the instance we are about to cite is a typical Freudian interpretation, much as Menzenrath may deny it.

Menzenrath[35] had begun writing to a friend,

> "Dass Sie bald nach . . . zu kommen
> gedenken, macht mir besonders Vergnügen,

but instead of besonders he wrote beden—i. e., he had started to say "Bedenken" which he explains as a complex due to the fact that the pleasure was not "particularly" great, and the whole note was an idle conventional form. That may well be, but Menzenrath had not noticed that the word "gedenken" preceded the unfinished "bedenken." Here we have an assimilation pure and simple. Furthermore it is unlikely that the word begun was *Bedenken*, for then it would have been written with a capital letter. It is apparent that the word was begun as "besonders," but as soon as the stroke of the "e" in "be," was finished, there was a shift of attention, and as "be" is similar to "ge" in sound, and both are prefixes of "*denken*," it need not at all seem odd that the *denken* of *gedenken* should reverberate while the mind was wandering.

In a later discussion,[36] Menzenrath shows still less sympathy with Freud's "universal hypothesis," though he thinks it is impossible on the part of adversaries to disprove Freud's contentions on introspective grounds. That Menzenrath has exaggerated Freud's invulnerability on the matter of speech and writing lapses, will, it is hoped, become evident from our present study.

The trouble with the Freudians is that instead of seeking

[34] S. Ferenczi: *Internat. Zeitschrift für ärztliche Psych.* Vol. III, 1915, p. 123.

[35] P. Menzenrath: Psychopathologie de la vie journalière, *Archiv für die gesamte Psychologie*, vol. XXIII, 1912, p. 510. Report of the VI[e] Congrès belge de Psychologie et de Psychiatrie.

[36] P. Menzenrath: Fehler des Alltagsleben: *Kongress für Experimentelle Psychologie*, VI, Göttingen, p. 57

an explanation in the context, in the immediate mental content, they soar off into the realm of the fantastic for their search, and return with a far-fetched interpretation. *As a methodological principle in the study of lapses, the writer would lay down the rule that first the word, sentence, or sentences preceding or following are to be examined,* then we must look into possible associations that may have determined the mistake; only in default of such clues, would it be legitimate to hunt for a new principle of explanation.

The Freudians scarcely ever give us the whole context of a lapse, but prefer to concoct a solution of the home-made brand and, it may be added, for home consumption. In the *Psychopathology of Everyday Life,* there are a number of acute observations, but in a great many statements there is neither rhyme nor reason, and in general Freud's doctrine must be taken *cum grano salis.* To be consistent, he should hold that *a workman who misses his step on a high scaffold and falls into space has a secret desire of ending his life; for in what way is the psychological mechanism of such a slip any different from others that bring no grave consequences with them?*

Similarly, the person who has taken poison by mistake—let us say the toper who reached out for the bottle of wood alcohol instead of the rum—is afflicted with a *taedium vitae.* In fact, many serious accidents, countless misfortunes are caused by slips. Are we to conclude, then, whenever some evil befalls us that somewhere in our nervous system there is a hankering after the mishap? Much of our illness is due to what is ordinarily spoken of as thoughtlessness. We escort our friend to the door on a cold night and unmindful of the fact that we have just left an overheated room, we stand in the draft conversing until we catch cold. Where disease is contracted through infection or contagion, lapses are nearly always at the root of such contractions. Yet we shall have a difficult task finding the man who may be said to invite illness, however unconsciously, unless as very rarely happens, when illness is a boon alternative as compared with some greater evil that may be in store for him.[37] As a rule, however, there does not seem

[37] *Cf.* A German Deserter's War Experience, p. 163. . . . "We were especially forbidden to make use of woolen blankets, because the French were infected with scabies. 'Scabies' is an itching skin which it takes at least a week to cure. But the order had a contrary effect. If one was the owner of such an 'itch blanket' one had a chance of getting into the hospital for some days." . . . "In the evening we took some of the forbidden blankets, hundreds of which we had captured that day . . . everybody wanted to get the 'itch,' however strange that may sound."

to be the slightest motive for wishing to fall ill or for courting death Even the motives that are thought to produce the questionable Oedipus and Electra complexes cannot be exploited in this connection.

Freud has been so fortunate in the authority he wields among his devotees that his utterances are accepted uncritically. On the other hand, most of the criticisms directed against Freud have come from men who dislike his doctrine on general principles, but who have not taken the trouble to subject his illustrations to a rigorous examination. It is on this very account that we have spent so much time on the discussion of Freud's analysis of lapses. His doctrine should further receive its *coup de grâce* by the trend of our experimental results[38] which show that lapses have been made in the hundreds; lapses in numbers, nonsense syllables, characters of the alphabet and other symbols. Shall we posit hidden complexes for each of them? But a given word or syllable may have been misspelled in many different ways by the same subject. The whole idea of looking for unconscious motives in experimental work of this sort seems too absurd to barely mention. In what way, then, are the lapses occurring in everyday life to be regarded as of a different material?

The principle of *determinism* which forms the background of the Freudian theory of lapses is sound, of course, *per se*. No scientist would deny that the writing lapse is conditioned by certain physiological or psychological antecedents. But what right has one to *create* a cause when the direct antecedent is in most cases apparent. Occasionally, as Wundt has recognized, there may be a complication of causes which it would be almost impossible to disentangle, and psychical influences may be at work,[39] but Wundt is by no means a Freudian even in the matter of speech lapses, though Freud eagerly seizes on a quotation from the *Völkerpsychologie* as a pretext for dragging in "Saul among the prophets," and by italicizing the phrase *"following the principle of the complication of causes"* in the passage quoted from Wundt the Viennese psychopathologist has evidently attached a special significance to Wundt's words, thus leaving the reader mystified.[40] In this state, the latter is apt to believe that Wundt has made an important concession to Freud.

[38] This study, which will shortly be published, constitutes the second part of the *Interference of Will -Impulses* previously referred to.

[39] Wundt, Völkerpsychologie, die Sprache, Vol. I, part I, pp. 361-382. 1900.

[40] Freud: Psychopathology of Everyday Life (English translation), pp. 79-80.

Well, then, let it be pointed out that the quotation mentioned has been broken off abruptly just where Wundt proceeds to illustrate precisely what he means by "a complication of causes." It certainly has no reference to any suppressed wishes or complexes, but to the *ordinary processes of association,* stimulated perhaps by the initial syllable of another word, or to several of the processes, already explained, taken collectively.

Not the slightest trace of adherence to, or rather anticipation of, Freud's principles can be detected in Wundt's discussion of lapses; and it is idle to read into his views such an interpretation. Moreover, it occurs to us that if Freud misinterprets a simple passage written in his native tongue, then how much more prone is he to put a wrong interpretation on phenomena as variegated as they are elusive? And if the master is apt to err so profoundly in an ordinary matter, then what shall we say of the epigones who lack his penetrating insight and generalizing ability as well as his systematic grasp of the phenomena generally classed under the subconscious?

ON SOUND DISCRIMINATION IN DOGS.

By W. T. Shepherd

The present paper is a report of experiments made on dogs, to ascertain whether or not they are able to discriminate sounds; viz: differences of musical notes of different pitch. It seems fair to conclude that if an animal forms an association between a certain musical note and food, so that he reacts in a particular manner to that note to obtain food, and does not so react to other notes, he discriminates that note from the other notes.

The reader may recall that Kalischer[1] reported experiments on dogs on auditory discrimination. In those tests, positive results were obtained by him. His method was to sound a certain note on an organ or harmonium; the dog would thereupon react in a definite manner, e. g. springing up and snapping at a piece of meat held in the experimenter's hand. Selionyi,[2] using a form of the "Pawlow method," got evidence of discrimination by dogs of the tones of an organ, of organ pipes and of the sounds of two whistles. Both Kalischer and Selionyi were chiefly interested in sound discrimination from the standpoint of physiology. The present writer in experiments reported by Cole,[3] obtained evidence of the discrimination of pitch by raccoons. In experiments I made in 1909 on auditory discrimination in Rhesus monkeys,[4] evidence of such discrimination by the latter animals of pitch and also of "Noise" was obtained. I also obtained evidence of discrimination of musical pitch and also of "Noise" in experiments made with cats.[5]

In the experiments herein reported, two animals were tested. Both were female beadle dogs. One, Kate, was about

[1] Kalischer, O. Eine Neue Horprufungsmethode bei Hunden. Sitz. d. Kgl. Ak. d. Wiss, X. 1907, 204 ff.

[2] Selionyi, G. P., Contributions to the Study of the Reactions of the Dog to Auditory Stimuli. St. Petersburg, 1907.

[3] Cole, L. W., Concerning the Intelligence of Raccoons. Jour. Comp. Neur. and Psy. 17; 236.

[4] Shepherd, W. T., Some Mental Processes of the Rhesus Monkey, Psy. Rev. Mon. Sup. No. 52, 1910, 26 ff.

[5] Shepherd, W. T., On Sound Discrimination by Cats. Jour. of Animal Behavior, Vol. IV. No. 1, pp. 70-75. 1914.

a year old; the other, Maud was about 8 years old. The younger animal appeared to be the more intelligent, though neither seemed conspicuously so. As far as I am aware, neither dog had had any previous training in sound discrimination.

In these experiments, as in those on raccoons, monkeys and cats, I used a method somewhat similar to that employed by Kalischer. The dog was placed in a box with a wire netting at the front and on the top. I sat at a distance of a meter from the cage and sounded the note on the instrument. The animal was to rear up on the netting at the front of the cage at the sound of a certain note, or rather after, as high, be fed at that note, and not to rear up at the sound of the other note, as low, and not to be fed at the latter note. At the sound of the "Food note," the dog was given a morsel of food, whether or not it reared up at that note. At the other note, it was not fed. The stimulus was food; proper reaction was accepted as evidence of discrimination by the animals of difference of pitch of different notes. Ten seconds were allowed for response. In experiments I and II, an ordinary harmonica was used; in experiment III, an organ (Estey). In each test, care was taken not to give by looks, movements, expression or in any other manner, any inkling of the proper response. As a further control, in a part of the tests, the experimenter sat out of sight of the animals. Furthermore, in order that the dogs should not react to the mere *rhythm* of the sounds, the notes were sounded in an irregular order.

I. Discrimination of a Difference of Three Octaves of Pitch on a Harmonica A4 Food Note, A1 Non-Food Note.

Kate—On the 3d day, in the 41st trial in all, the animal first reacted correctly, in the 11th trial of 15 that day, i. e 15 on each note. In that day's trial she seemed to *notice* and *watch,* somewhat, as if interested. No further indications were noted or evidence of any sense of difference of tones being distinguished by her until the 6th day. Then, she reared up at note A4 4 times in the 15 trials. The next day, she reacted to the high, A4 note, 5 times. On the 8th day, she responded correctly 6 times and incorrectly 5 times. On the 9th day, there were 7 correct reactions and 3 incorrect ones, i. e. 3 to A1. By the 11th day, of the 15 trials, there were 11 reactions to A4, and but 2 to A1. By the 24th day, or in 300 trials in all, the dog's discrimination was perfect. In the 10 trials of each auditory stimulus, 10 correct responses were given by her, and no responses to A1. She was then given a final "Test" of 10 trials of each note and made no errors.

Maud—The older dog, in 375 trials extending over many days, showed no definite indications of discrimination of the difference of three octaves of pitch. In a few trials of the 375, she reacted correctly, and in a few others gave reactions which may be regarded as doubtful, e. g. on a few occasions, though she did not rear up at A4, she *looked up*, and appeared to show some little indication of knowing the food note.

II. Discrimination of a Difference of One Octave of Pitch on a Harmonica. Fed at Low, A1.

Kate—In the first day's trials, upon changing the food note from A4 to A1, the record seems to show that Kate was puzzled. In 10 trials of the notes, she reacted to each 5 times. On the 3d day, she responded to A1 6 times and to A4 3 times; on the 4th day, to A1, 5 times and to A4 1 time. She was forming the association. On the 8th day, 80 trials of each sound in all, she was correct 9 times and wrong 1 time. On the 12th day, she made no mistakes. On the 14th day, right 9 times, wrong 1 time. 140 trials were made in all on each note in the one octave test. Kate did not seem to get this discrimination so nearly perfect as in the three octave test. Possibly, more trials would have so perfected her.

III. Discrimination of Three Octaves on an Organ; i. e. the Difference between F.1, Bass Clef and F.3 Clef.

In five days experiments of 10 trials each of each tone, daily, no evidence was obtained of discrimination of the notes. Neither were any indications observed of the beginning of such discrimination. While in some of the trials the animal reacted to one or the other of the notes, the record shows that in the last three days trials, 30 on each tone, no responses to either were obtained.

However, only 50 trials on each note were made in all, and it is quite possible that if many more than that number had been made, discrimination would have been shown. It required 300 trials in the first harmonica test and 140 in the second. Furthermore, it is but fair to bear in mind that the tests with the organ were made in the afternoon, while those with the harmonica were made in the forenoon, as it happened, and the dogs were usually fed at noon. Therefore, in the former case there not being present the stimulus of hunger as in the latter, we could hardly expect so much interest on her part, and so not such aptitude for discrimination. I believe that with further trials, under the same conditions of hunger as a stimulus, as in the harmonica tests, discrimination would here also have been shown.

From these experiments we may conclude that dogs, or at least some dogs, discriminate differences of musical pitch. Our record of Kate's work shows this. Furthermore, her looks, at the time, actions, etc., gave clear indications of such discrimination to any unbiassed observer, e. g. *Getting down quickly,* after getting up at the wrong note.

It may be of interest to compare our results here with those obtained with other animals. Kate required 300 trials to perfect her discrimination of a difference of three octaves of pitch. One of the cats required 90 and the other 45 trials for two octaves. The two raccoons required respectively 100 and 150 trials. Two Rhesus monkeys required only 30 and 40 trials respectively to discriminate two octaves difference. In each case only two individuals were tested. Therefore, conclusions as to comparative rapidity of forming associations, and ability of discrimination are not strictly warranted.

CONFESSIONS OF AN AGORAPHOBIC VICTIM

By Vincent

For some time I have been planning to commit to writing personal observations of my condition, sensations and experiences during a long period of suffering from a malady which, for lack of a better name, medical men have termed "Agoraphobia"—fear of an open place.

As I am unacquainted with medical literature I do not know how much has been written on this subject. Only one case of "confessions" has come to my notice. Some time ago I read several pamphlets on "Religion and Medicine"—Emmanuel Church publications—one of which discussed various "nervous" disorders, among them the "phobias." In the course of the discussion there was introduced testimony of a man who had been grievously afflicted with agoraphobia. I devoured these "confessions" with the greatest avidity. It was the first and only time I had read any testimony of an individual thus afflicted.

This same pamphlet stated that the man had since become almost cured. Encouraged by these good tidings I tried to practice mental poise and tranquillity for several weeks. During those few weeks I saw wonderful improvement in my condition. However, I lapsed gradually into my old habit of self-neglect, and with this neglect improvement in my health ceased.

The testimony taken from the above mentioned pamphlet leads one to infer that the affliction came upon the victim rather suddenly, and that the symptoms of the malady were present only occasionally: namely, when the man was on certain streets and at definite places on these streets.

If the above inference is correct I have to testify that my case is quite different. In the first place, my malady came upon me gradually and went through definite stages of development. Second, I am conscious of my affliction every minute that I am awake.

I am now in mid-life and I have not seen a well day since I was about twelve years of age. Before I experienced any of the symptoms of agoraphobia I recall that a strange affliction came over me, an affliction that seemed to baffle the country

doctors who were consulted. I was taken suddenly with "spells" which lasted about thirty minutes. During these attacks I was entirely conscious and rational. As I remember the affliction, a sort of chill came over me—not like an ordinary chill, but a sort of "coldness" that produced a very unusual sensation, or perhaps, a *lack* of sensation would describe it more accurately. I have an impression that the physicians suggested that it might possibly have been due to a temporary stoppage of circulation. At any rate the remedy applied was vigorous rubbing of my body with rough towels, or with the bare hands by those attending me; some times, when it was convenient, a hot bath was resorted to; stimulants were also administered. I was more liable to these attacks during times of excitement. For instance, I recall that one of the worst attacks I ever had came over me while I was attending the funeral of a relative. When there seemed to be no outward cause that brought on the attacks, it was noted by my parents that they recurred periodically; I believe it was every fourteen days or was it eleven days?

My own belief now is that the illness referred to was due to some disorder of the nervous system. At any rate, after a few months I outgrew the tendency. However, I was not a well boy. I was abnormally timid and more or less melancholy, and was given to worry and brooding.

In this connection I would remark that I was born with an active, nervous temperament, and was always, as a boy, more or less timid. I was abnormally alert when there was possibility of danger. I remember how I used to run and leap like a fawn when passing through tall grass and weeds in the summer time, fearing lest I should encounter snakes, which were rather common.

When my strange illness came upon me I worried over it, fearing that I should die in one of the attacks. During this overwrought state of mind I was much affected by a terrible tragedy which took place in our community. One of my playmates, a boy about eleven years of age, disappeared one day. The supposition was that he had fallen into the river and had been drowned. On this theory, the river was dragged day after day for more than a week, but no body was discovered. Then the theory was advanced that he had been kidnapped; or perhaps, he being a venturesome lad, had started out to "see the world" and had met with foul play. I recall very well how the entire community was stirred and what effect it had on the boys and girls of the village and neighborhood. Finally, one morning in late autumn, the body of my playmate was found

on the bank of the river, at a bend in the stream just below the village. But he had not been drowned—his throat was cut from ear to ear. The murder had been committed in a cellar in the village by a half-crazed woman who had later carried the body, by night, and placed it on the bank of the river.

This whole affair had a most depressing effect upon me. After that I almost feared to be alone, was afraid to go to the barn in the day time, and suffered when put to bed in the dark. Perhaps the worry over my illness, together with the depressing effect of the tragedy, brought on a severe attack of nervous dyspepsia. This resulted in mal-nutrition for my over-sensitive nervous system which, perhaps, laid the foundation for the "phobias."

It was during the months which followed that I remember having experienced the first symptoms of agoraphobia. There was a high hill not far from my home in the country where we boys used to coast in the winter time. One evening while coasting, in company with other boys of the neighborhood, I experienced an uncomfortable feeling each time we returned to the top of the hill. It was not a well defined symptom of this horrible (I use the term deliberately) malady, but later experiences have taught me that it possessed the unmistakable earmarks. As the months went by the symptoms developed, with the result that I avoided hill-tops, so far as possible.

Later, perhaps a year or so, I commenced having a dread of wide fields, especially when the fields consisted of pasture land and were level, with the grass cropped short like the grass on a well-kept lawn. I likewise commenced to dread high things, and especially to ascend anything high. I even had a fear of crowds of people, and later of wide streets and parks.

I have outgrown the fear of crowds largely, but an immense building or a high rocky bluff fills me with dread. However the architecture of the building has much to do with the sort of sensation produced. Ugly architecture greatly intensifies the fear.

In this connection I would remark that I have come to wonder if there is real art in many of the so-called "improvements" in some of our cities, for, judging from the effect they produce on me, they constitute bad art.

But the one thing that I would make plain is that the malady is always present. As I write in my study I am painfully conscious of it—in fact, I am conscious of it during every hour that I am awake. The fear, intensified, that comes over me

while crossing a wide street is, it seems to me, an outcropping of a permanent condition.

It is not pain that I feel, but it seems to me that it is more than a dread. I am not nervous, as some people whom I know — I mean in the same way, but it certainly is a case of "nerves." Let me illustrate:— I enter a home and sit in an arm-chair chatting with my friend; I soon find myself gripping the arm of the chair with each hand. My toes curl in my shoes, and there is a sort of tenseness all over my muscles.

At times my phobias are much more pronounced than at other times. Sometimes, after a strenuous day, on the following morning, I find myself almost dreading to walk across a room; at other times I can cross a street without any pronounced discomfort. Manual labor improves my condition. Walking and riding horseback are beneficial.

Usually I feel better in the evening than in the morning, partly because the darkness seems to have a quieting effect upon me. I love a snow storm a regular blizzard, and feel much less discomfort in going about the town or riding on a train on such days, probably because one's view is obstructed. In fact I welcome stormy days, strange to say, with a zest that is hard to appreciate; in short, some of the most stormy days of the hard winters of this region stand out as bright spots in my life. On such days I make it a point to be out and about the town.

I dread going out on water in a boat, especially if the surface is smooth; I much prefer to have the waves rolling high. The most restful place in all the world for me is in a wood, where there is much variety in the trees and plenty of underbrush, with here and there low hills and little valleys, and especially along a winding brook. I love "the quiet places by the woods." Also the little lakes with their narrow bays and wooded shoreline. I love quiet, restful landscape. It seems to rest my eyes and soothe my spirit. On the other hand, let the landscape be bold and rugged and bleak and it strikes terror to my soul.

I lived in New Haven during four years, while a student at Yale, and never climbed to the top of East Rock. And the big Green near the university always made me very uncomfortable when I looked at it.

I ride a bicycle along streets with comparative comfort where I should suffer agony were I to walk. In walking I feel less uncomfortable in passing along the street if I carry a suit-case or traveling-bag — something to grip.

When I think of the agony which I have experienced for many years I am astounded at the endurance of the human spirit. Let me illustrate:— I have such a dread of crossing a long bridge on foot that it would require more courage for me to walk to the part of my town situated across the river than it would to face a nest of Boche machine guns. And yet day after day, month after month, and year after year I have carried in my soul the dread of such an eventuality.

No one knows the truth about my condition. It is one of the characteristics of the victim of the disease to conceal it most cunningly. I think I am an honest man in all essential things. My credit is good at the banks. But I have deliberately told lies to avoid embarrassing situations and have even changed my plans to have my lies "come true."

I have never mentioned my condition to a physician. I have passed several examinations for life-insurance policies—in fact, have never been refused a policy by any life insurance company. I eat and sleep well, am rather strong and wiry physically. My occupation makes heavy demands on the vitality and entails considerable mental exercise, but I am seldom unable to take up my duties on account of indisposition. However, in my own mind I am a nervous wreck, weak, worthless, and unworthy of the high respect which the community accords me.

In spite of all this I seem to exercise marked power of leadership in my town, and am known as a public speaker of ability.

Of course, the paramount question with me is: Is there hope of a cure? Can I ever take my place in the world unhandicapped as other men are, and enjoy a single day undepressed by dark dread? If I could be as other men, it seems to me that my usefulness should be increased a hundredfold. Those who have not been thus afflicted cannot understand just what I mean.

I see a man hobbling past my house on crutches, a cripple for life, and I actually envy him. At times I would gladly exchange places with the humblest day-laborer who walks unafraid across the public square or saunters tranquilly over the viaduct on his way home after the day's work.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXXVII. Directed Egocentric Reactions

By Katherine B. Graves, Evelyn Heath, and M. F. Washburn

It is unnecessary to point out the great need of tests which will reveal the emotional and social characteristics of individuals, and the great difficulty which attends the discovery of such tests. The present study has no striking success in this line to record. It is, however, perhaps worth publishing as the account of an attempt.

The free association reaction experiment of Jung has been regarded as furnishing some indications of the affective and social characteristics of a given individual. Wells[1] has given us a modification of Jung's reaction types, in which he classes certain responses as egocentric. The group of egocentric responses includes reactions with a predicate or adjective, reactions with a proper name or pronoun, and perseverations. All of these reactions are taken as indicating a subjective tendency on the part of the individual who makes them; a tendency to self-reference.

Our attempt in the present study was to see how far an individual's tendency towards egocentric reactions, thus defined, in the free association experiment, correlated with the speed with which she could associate personal experiences with a stimulus word when expressly instructed to do so. It would seem possible that a person who is subjectively minded and has a tendency to self-reference might, if given a series of stimulus words with the instruction to react to each word as soon as it had suggested a personal experience, make such associations with especial speed. This would be on the supposition that such an egocentric person would have personal reminiscences always on tap, so to speak.

The materials used were a series of one hundred stimulus words, all nouns, including the nouns from the Kent-Rosanoff list; a second series of fifty nouns; and a stop-watch. The observers were given the following instructions in the preliminary free association series with the one hundred words as stimuli: "I shall pronounce to you a word, and you are to respond with the first word that occurs to you." The watch was in every case started as the stimulus word was pronounced, and stopped as soon as the observer made any verbal response. The reaction word and the reaction time were recorded. The times were averaged for the hundred words, and the number of reaction words in each of the following classes was counted: adjectives not emotional (such as 'green'), emotional adjectives (such as 'pretty'), proper names, pronouns, and perseverations. In the second part of the experiment the fifty-word series was used, and the instructions were as fol-

[1] A Preliminary Note on The Categories of Associative Reactions, *Psych. Rev.*, 18, 229-233 (1911).

lows: "This time do not try to react with a word, but say 'Yes' as soon as the stimulus word has suggested to you something connected with yourself." The reaction times were recorded and averaged. By not requiring the observer to react with a word in this part of the experiment, it was intended to avoid the delay that would have been involved in making a choice of words. When a personal experience is recalled, it is often hard to select a single word that will represent it. We found the average time required for each observer to respond with the 'Yes' that indicated the recall of a personal experience, and recorded the difference between this average time and the same observer's average time of reaction in the free association series that preceded. This difference was regarded as measuring the ease and promptness with which the observer in question could make a personal application of the stimulus word, and thus as a measure of the 'egocentricity' of the observer. We then undertook to correlate this measure with the number of so-called egocentric reactions made by the observer in the free association series.

This was done for fifty-one young women observers. The first and last quartiles according to the amount of delay in making the egocentric references were listed: thirteen observers in each quartile. We first found the number of predicate or adjective reactions that were made in the free association series by each of these quartiles. The quartile of greatest delay, which might therefore be regarded as the least egocentric quartile, gave a total of 71 emotional adjective responses, an average of 5.4, and a range of from 1 to 11. The quartile of least delay, hence the most egocentric quartile, gave a total of 77 emotional adjectives, an average of 5.9, and a range of from 0 to 24. The 24-case was quite exceptional. It appears from these results that our egocentric observers were no more inclined to use emotional adjectives as reaction words in free association experiments than were our non-egocentric observers.

The quartile of greatest delay (least egocentric) gave a total of 173 adjective or predicate responses of all kinds; an average of 13.3 and a range of from 2 to 37. The quartile of least delay (most egocentric) gave a total of 228, an average of 17.5, and a range of from 1 to 45. This indicates that our more egocentric group was slightly more inclined than our non-egocentric group to predicate reactions in general.

The quartile of greatest delay (least egocentric) gave in the free association experiment a total of 259 perseverations; an average of 19.9, and a range of from 8 to 28. The quartile of least delay (most egocentric) gave a total of 251 perseverations; an average of 19.3, and a range of from 9 to 34. There thus appears to be no difference between our egocentric and non-egocentric observers as regards the number of perseverations in a free association series.

Finally, the quartile of greatest delay gave in the free association reactions a total of 42 responses with proper names and pronouns. The average was 3.2, and the individual figures were 4, 7, 0, 18, 3, 4, 0, 0, 3, 2, 1, 0, 0. The quartile of least delay gave an average of 7.6 responses with proper names and pronouns; the individual figures were 5, 16, 18, 5, 19, 3, 2, 0, 17, 8, 6, 1, 0. There is thus a noticeably greater number of responses with proper names and pronouns in the case of observers who are especially prompt, when directed to do so, in recalling personal experiences.

The conclusion suggested by these experiments is, then, that there exists a certain degree of correlation between the tendency to give predicate reactions and reactions with proper names and pronouns in

a free association series, on the one hand, and promptness in applying another set of stimulus words to personal or egocentric matters. This correlation seems not to exist between emotional adjectives as a special class and promptness in making personal reference; nor between the number of perseverations and the speed of personal reference. There is a noticeable correlation between the tendency to give proper names and pronouns as reaction words in a free association series, and the tendency to make a personal application of another set of stimulus words when directed to do so.

XXXVIII. An Attempt to Test Moods or Temperaments of Cheerfulness and Depression by Directed Recall of Emotionally Toned Experiences

By Eleanor Morgan, Helen K. Mull, and M. F. Washburn

In this test five lists of fifty stimulus words each were used. The experiment was performed on five successive days with each observer, a different list of words being used on each day. The instructions were always the following: "When I pronounce a word to you, observe what idea that word first calls to your mind, and report whether it is a pleasant or unpleasant idea. If it seems neither pleasant nor unpleasant, but indifferent, continue thinking until either a pleasant or unpleasant idea is suggested, and report which it is." The observer's eyes were always closed to prevent suggestions from the surroundings.

The idea on which the test was based was that, according to the law which James called that of emotional congruity, a person in a state of exaltation would think of more pleasant ideas in connection with a stimulus word than would a person in a state of depression. If a person on five successive days maintains a high level of, say, pleasant associations, it may be plausibly suggested that such a person is either steadily cheerful in temperament, or is passing through a somewhat prolonged mood of cheerfulness. The first requisite is evidently the establishment of a norm showing the number of pleasant and unpleasant suggestions that a given stimulus word may be expected to produce.

These norms were found in the following way. The number of cases where a pleasant idea was the first one suggested by the stimulus word was counted for each observer and each series of fifty words; the average number of pleasant suggestions for the total group of ninety-seven young women observers was then found for each of the five series of fifty words. Thus for Series I, the ninety-seven observers furnished on an average 33.7 cases out of the fifty, where a pleasant idea was the first one suggested; Series II an average of 34.8 cases; Series III an average of 34.4 cases; Series IV an average of 36.1 cases; and Series V an average of 35.9 cases. The stimulus words which formed the series were originally chosen at random; they comprised both nouns and adjectives, and no especial care was taken to select those with or without pleasant or unpleasant suggestions. The averages just given show that the proportion of pleasant to unpleasant words in each series was very nearly the same, and that in all the series there were more pleasant than unpleasant words. It seems to be a psychological fact that a normal person recalls pleasant experi-

ences more readily than unpleasant ones; this law may have operated not only in the minds of the observers, making them think of more pleasant than unpleasant ideas in connection with the stimulus words, but also in the mind of the author of the method, whose intention was to choose the words at random. Among the observers, out of four hundred and eighty-five cases, five series for each of ninety-seven persons, only nine experienced more unpleasant than pleasant suggestions.

The optimism, temporary or permanent, of a given observer was now measured in the following way. The difference between the number of pleasant suggestions she obtained from a given series and the average number obtained for that series, whether the difference was positive or negative, was found; and this was done for each of the five series in the case of the observer under consideration. The algebraic sum of these differences, if it was large enough, might be considered as suggestive of optimism or pessimism on the observer's part. Thus a person the sum of whose differences from the average amounted to plus 53 might be regarded as optimistic; one for whom the corresponding value was minus 40 might be classed as pessimistic.

Whether the method really tested temperament, however, depended on whether its results could be found to correlate with some other measure of optimistic or pessimistic tendencies. No other method suggested itself except the estimates of personal friends of the observers. Accordingly, for each of eighty-nine observers judgments were obtained from three intimate associates, the two questions put being, "Is A.B. inclined to be optimistic and cheerful, or pessimistic and depressed most of the time? Is she steady or fluctuating in mood?" Needless to say the judges in each case gave their estimates without consultation together.

The results may be expressed in the following way. There were twenty-one observers who in all five series gave a number of pleasant associations which was above the average. We may call them, on the hypothesis that this test indicates optimism, the steadily optimistic observers. Of these, eleven were reported by all three judges who were consulted in their respective cases, as steadily optimistic in temperament. Three of the group were reported by all three of their judges as variable in temperament but inclining to optimism. Two were judged steadily optimistic by two of their judges and indifferent, neither optimistic nor pessimistic, by the third judge. One was described as steadily optimistic by two judges and as variable tending to optimism by the third. One was reported by all three judges as fluctuating in temperament, and one as steadily neutral or balanced. One was reported by two judges as variable tending to pessimism and by one as neutral.

There were fifteen observers who in all five series gave a number of pleasant associations which was below the average, and who therefore may be tentatively called the steadily pessimistic group. Of these one only was reported by all three judges as steadily optimistic; one was reported steadily optimistic by two judges and variable to optimistic by the third. Three were reported as steadily optimistic by one judge and as variable to optimistic by two. One was described as steadily neutral or indifferent. One was reported steadily pessimistic by one judge, indifferent by the other two. Three were described as steadily pessimistic by two judges and as steadily optimistic by the third. Three were reported steadily pessimistic by two judges and

indifferent by the third, and one was reported steadily pessimistic by three judges.

An examination of these figures strongly suggests the existence of a real positive correlation between exceeding or falling below the average number of pleasant associations in five successive days, and the judgments of a person's intimate associates regarding his temperament. It should be noted that not only were more of our observers "steady optimists" than "steady pessimists," but their judges were still more unwilling to call them pessimistic. Only one of the eighty-nine observers who were judged by three friends each was reported as steadily pessimistic in temperament, although fifteen showed a steady deficit of pleasant associations on five successive days.

The method needs and is to receive further investigation: in particular, we plan to secure greater accuracy and uniformity in the conditions under which the estimating judges work.

XXXIX. The Healy-Fernald Picture Completion Test as a Test of the Perception of the Comic

By Miriam A. Walker and M. F. Washburn

While examining the Healy-Fernald Picture Completion Test its possibilities as an apparatus for investigating the perception of the comic occurred to the senior author of this study. The picture, it will be remembered, represents a variety of independent occurrences, and in the representation of each some essential object is missing. A number of blocks are supplied which can be fitted into the empty spaces where the missing objects have been; some of these blocks bear the missing pictures, others have pictures of irrelevant objects. When the puzzle is used as originally intended, the problem for the child tested is to find and put in place the proper object for each episodic picture. Thus in the blank space between the boys whose attitudes show that they are playing football the square block bearing the picture of a football must be fitted; the space in front of the child who is holding a saucer of milk must be filled with the picture of the approaching cat; below the boy dropping an apple out of a tree must go the picture of the basket of apples; opposite the fighting rooster must go the other fighting rooster; the obviously terrified boy must have the savage dog supplied, and so on. There are ten such episodes. The drawings themselves are comic; the exaggerated expressions make the effect amusing even when the missing pieces are correctly supplied.

But in some of the episodes at least the effect is much funnier if a wrong object is substituted; and this suggested the use of the puzzle as a test of the comic. If instead of the football there is put the picture of a crying baby, which thus appears to be tossed to and fro; if instead of the apple basket a comfortably sleeping cat appears about to be hit by the falling apple; if a pile of school books is supplied in place of the dog from which the small boy flees in horror; a new joy is given to contemplation of the varied scene.

Now all authorities agree that a situation to be comic must involve an element of incongruity. We may however perhaps distinguish as belonging to different intellectual levels the comic that is based on mere incongruity, such as we find in nonsense rhymes, and that where

the incongruity is combined with appropriateness and thus with an element of wit. Thus to fit in a picture of a robin instead of that of the savage dog, so that the small boy flees from a harmless bird, is to make a merely incongruous combination; to put in the bundle of schoolbooks introduces the element of appropriateness and wit, since it hits at the too frequent emotional attitude of small boys towards school books. To put in place of the football a lighted candle gives mere incongruity; to put in the crying baby may do violence to the maternal instinct, but the baby's expression is undoubtedly appropriate. It seemed worth while to investigate whether any significant differences exist between individuals in their susceptibility to the humor of the purely incongruous and that of the incongruous which involves also the appropriate.

The following method was used. For each of the vacant squares on the board three picture squares were selected. One of these was the appropriate one, which logically completed the picture; one formed a merely incongruous combination with the picture; one was intended to appeal by an element of appropriateness in the midst of incongruity to a more intellectual sense of humor. It was not possible to find a completion that fulfilled this condition for all the blank spaces on the board. The picture squares that were used as 'merely incongruous' were as follows: for the broken window, cherries; for the barking dog, the robin; for the cat, the old shoe; for the football, the candle; for the little girl's hat, the pumpkin; for the cartwheel, the bunch of flowers; for the basket of apples, the pipe; for the piece of wood, the pocket-book; for the bird, the glove; for the rooster, the bottle of milk. The "really funny," as opposed to "merely incongruous" blocks, were as follows: for the dog, the bundle of books; for the football, the crying baby; for the basket of apples, the sleeping cat; for the rooster, the clock (which gave the other rooster the air of being horrified to discover how late it was; perhaps a rather subtle bit of humor when compared with the baby as football). Recognizing that the following combinations were not really funny, we used them for want of anything better in the remaining episodes; for the broken window, the whole window; for the cat, the fish (our observers, as will appear, thought this a highly humorous combination); for the hat, the bottle of medicine; for the cartwheel, the necktie; for the piece of wood, the cup; and for the bird, the mouse (this also appealed to our observers' sense of humor).

The following directions were given to the observer: "I am going to put three blocks successively in each of these squares, and I want you to assign a numerical value, from 1 to 5, 5 being the highest, for the degree of funniness of each of the blocks." The order followed was: the appropriate block; the "really funny" block; the "merely incongruous" block.

Eighty young women college students, eighteen seventh grade boys and girls, and eighteen fourth grade boys and girls were the observers.

Out of the data collected, several rather interesting conclusions and suggestions for further work emerge. (1) What may be called the *intensity of the reaction to the comic* of the type presented in these experiments is *greatest in the fourth grade children, less in the seventh grade children,* and *least in the adults.* This factor of intensity may be measured by the average numerical value assigned to the funniness of each of the combinations, by each of the three groups of observers. The *college students* found an average numerical value of *three or above* for *six* combinations only; the baby for the football (3.9); the

mouse for the bird (3.6); the fish for the cat (3.5); the sleeping cat for the basket of apples (3.5); the school-books for the dog (3.4) and the clock for the rooster (3.4). The *seventh grade* children estimated the funniness of *eight* of the combinations at an average of *three or above;* the baby for the football (4.2); the sleeping cat for the basket of apples (3.8); the mouse for the bird (3.6); the broken window in its proper place (3.5); the barking dog in his proper place (3.4); the cup for the piece of wood (3.3); rooster in his proper place (3.1), and the necktie for the wagon wheel (3.1). The *fourth grade* children estimated *ten* combinations at an average of *three or over;* baby for football (4.6); rooster in proper place (4.3); cat for apples (4); mouse for bird (4); wheel in proper place (3.8); cat in proper place (3.5); dog in proper place (3.4); broken window in proper place (3.2); clock for rooster (3.1); necktie for wheel (3).

(2) The pictures *in their appropriate context* were *funnier to the fourth grade children than to the seventh grade children, and to the seventh grade children than to the college girls.* The average score for all the appropriate pictures was for the *fourth grade* group 3.14; for the *seventh grade* group 2.54; for the *college girls* 1.73. Since these figures, because of the small number (18) in each of the seventh grade and fourth grade groups, are not very significant, they may be supplemented by the statements that in the seventh grade there were thirty-five cases where the appropriate picture was held to be funnier than either the 'really funny' or 'merely incongruous;' in the fourth grade there were seventy-four such cases. In four groups of eighteen college girls each, taken in the random order in which they were experimented upon, the number of such cases was 22, 24, 36, and 13 respectively. The distribution of the tendency to find the appropriate picture funnier than any other may be indicated by the following figures. In the *fourth* grade three observers gave seven cases each where the appropriate picture was judged funniest; one observer gave six such cases; eight observers gave five cases; two gave four cases, three gave three cases, two gave two cases, none gave only one case, one gave no case. In the *seventh* grade, no observers gave either seven, six, or five cases; four gave four cases each; three three cases each, four two cases, two one case, and five no cases. In *Group I, college girls,* one gave five cases; one four cases; one three cases, two two cases, six one case, seven no cases. In *Group II, college girls,* one gave five cases, two four cases, one three cases, two two cases, four one case, eight no cases. In *Group III, college girls,* one gave six cases, two five cases, three four cases, one three cases, two two cases, one one case, eight no cases. In *Group IV, college girls,* none gave four or more cases, two gave three cases, three two cases, one one case, eleven no cases. It would seem plausible to suppose that finding the appropriate picture especially funny may indicate a certain naïve quality of mind, a tendency not to be bored by commonplace situations, which would be oftener found in young children.

(3) A more unexpected result is that *mere incongruity is decidedly funnier to seventh grade children than to either fourth grade children or adults.* The average numerical value assigned to the pictures which were merely incongruous in their setting was for the fourth grade, 2.59; for the seventh grade, 3.34; for the college girls, 2.65. There were in the fourth grade group forty-two cases where the merely incongruous picture was thought to be the funniest completion; in the seventh grade there were seventy such cases, and in the four groups of college girls the numbers were forty-four, forty-one, forty-eight,

and forty-three respectively. In the fourth grade group, there was no observer who found the merely incongruous picture the funniest completion in more than four cases out of the possible ten; there were two who found it so in four cases, five who found it so in three cases, eight who found it so in two cases, three who found it so in one case. In the seventh grade, there were three observers who found the merely incongruous picture funniest in six cases out of the ten; two who found it so in five cases, four in four cases, four in three cases, three in two cases, no one who found the incongruous picture funniest in only one or in no case. In the first group of college girls, two found the incongruous picture funniest in six cases, no one in five, three in four cases, two in three cases, four in one case, two never. In Group II, college girls, no one found the incongruous picture funniest in six cases, two so found it in five cases, three in four cases, two in three cases, four in two cases, four in one case, two never. In Group III, college girls, one observer found the merely incongruous picture funniest in seven cases, no one in six cases, one in five cases, two in four cases, three in three cases, six in two cases, two in one case, two never. In Group IV, college girls, one observer found the incongruous picture funniest in seven cases, no one in six cases, two in five cases, one in four cases, four in three cases, four in two cases, two in one case, three never.

It will be seen that there is much more individual variation in the taste for the 'purely incongruous' style of humor among the adults than among the children. It is obviously desirable to extend the experiment to much larger numbers of fourth and seventh grade children, and this we plan to do.

XL. The Results of Certain Standard Mental Tests as Related to the Academic Records of College Seniors

By Hermine Baum, Miriam Litchfield, and M. F. Washburn

The tests used were the following:

(1) The Hard Opposites Test of Woodworth and Wells. The results were stated in terms of the average time for a single correct response.

(2) The Analogies Test, lists A and B, of the Woodworth and Wells set: the results were stated in terms of the mean between the average time for a single correct response in A and that for a single correct response in B.

(3) The Substitution Test, the Woodworth and Wells form, with the star, circle, square, cross, and triangle. The results were stated in terms of an index of efficiency, accuracy being reckoned by subtracting for each error two from a total of 100, and the time required for the whole sheet being divided by the index of accuracy thus obtained.

(4) The Cancellation Test, the form beginning hplg or that beginning zcyu , a's being cancelled. The results were stated in the form of the index of efficiency, for the work-limit method: 100 times the index of accuracy divided by the time; the index of accuracy being found by dividing the number of a's crossed by the number on the page.

(5) The Information Test. This was the familiar Whipple list of words, beginning 'ageratum.' Since an observer may often think he can define a word when he cannot, and his opinion can be refuted only by a laborious criticism of definitions, we took as the measure of an individual's information the number of words on this list that were marked N, as being wholly new and unfamiliar to her. One is not likely to be mistaken on such a point as this.

No preliminary practice was given on any of the tests except the Opposites and the Analogies Tests, where examples were given.

The observers were chosen on the following principle. They were all Vassar students in the second semester of their senior year. At this period the Dean's Office has on record for each girl a numerical value which represents both the quantity and the quality of her work for the seven semesters she has completed. For each semester hour of work done of grade A, she receives 5 points of credit; each hour of B grade work gives her three points, each hour of C work 2 points; each hour of D work (just passing) one point. The object of our study was to see how well the work of a student, thus measured, correlated with her performance in the above tests.

The study may be divided into two parts.

I. Thirty-eight seniors were chosen whose records formed a fairly continuous scale from the highest to the lowest in the class. The plan was to have their numerical standings differ by four points. For the lowest twenty-three on the scale, this was actually the case; in the upper half of the scale there was occasionally a greater distance between the ranking of one individual and the next above or below her. Beside this ranking as regards academic performance we set the rankings of the students in the various tests, and calculated rank difference correlations. The results were as follows.

Index of	Correlation	between	Cancellation	and	Academic	Rank,	+ .06,	P.E. .11.
"	"	"	Substitution	"	"	"	+ .03,	P.E. .11.
"	"	"	Opposites	"	"	"	+ .30,	P.E. .09.
"	"	"	Analogies	"	"	"	+ .39,	P.E. .09.
"	"	"	Analogies and Oppcaites combined	"	"	"	+ .40,	P.E. .09.
"	"	"	Information	"	"	"	+ .33,	P.E. .09.

Considering the very slight differences in academic rank on which the scale of academic ranks was based, the Analogies, Opposites, and Information Tests make a good showing.

II. Two groups of twenty-five students each were chosen. The academic ranks of all in group I were represented by numerical values over 350; the ranks of those in Group II were represented by numerical values under 210.

The results of the tests for these two groups, I containing the seniors with best records, II those with poorest records, were as follows:

Cancellation: average index of efficiency, Group 1, .69, m.v. 11.

Cancellation: average index of efficiency, Group 11, .74, m.v. 15.

The group of lower academic standing did somewhat better than that of higher academic standing. The distribution of individual scores confirms this: the numbers were as follows. Above 1, in Group I, 1; in Group II, 1. Between .9 and 1, Group I, 5; Group II, 3. Between .8 and .9, Group I, 3; Group II, 6. Between .7 and .8, Group I, 4; Group II, 7. Between .6 and .7, Group I, 10; Group II, 4. Between .5 and .6, Group I, 2; Group II, 3. Below .5, Group I, none; Group II, 1.

Substitution; average index of efficiency, Group I, 1.31, m.v. .13.
Substitution; average index of efficiency, Group II, 1.34, m.v. .17.

The smaller this index, the better the performance. Here the group of higher academic standing did a very little better. The distribution of individual scores is in accordance: The number of scores above 1.7 was, for Group I, none; for Group II, 1, Scores between 1.6 and 1.7 were made by 2 observers in Group I; by 3 in Group II. Scores between 1.5 and 1.6 were made by 4 in Group I and by 3 in Group II. Scores between 1.4 and 1.5 were made by 1 in Group I and by 5 in Group II. Scores between 1.3 and 1.4 were made by 6 in Group I and by 2 in Group II. Scores between 1.2 and 1.3 were made by 6 in Group I and by 3 in Group II. Scores between 1.1 and 1.2 were made by 4 in Group I and by 6 in Group II. Scores between 1 and 1.1 were made by 2 in Group I and by none in Group II. Scores between .9 and 1 were made by none in Group I and by 2 in Group II.

Opposites. In this test the smaller the score the better the performance.

Average time of a correct response, Group I, 2.87, m.v. .73.
Average time of a correct response, Group II, 3.58, m.v. 1.37.

Here the group of superior academic standing does distinctly better than that of poorer academic standing, and the distribution of cases bears this out. Scores of between 1 and 2 seconds, that is, the best scores, were made by four from Group I and by 2 from Group II. Scores between 2 and 3 seconds were made by 16 from Group I and by 7 from Group II. Scores between 3 and 4 seconds were made by 4 from Group I and by 7 from Group II. Scores between 4 and 5 seconds were made by 1 from Group II and by 2 from Group 2. Scores above five seconds were made by no one in Group I, and by 7 persons from Group II.

Analogies. Here also the smaller the score the better the performance.

Average time of a correct response, Group I, 2.46, m.v. .26.
Average time of a correct response, Group II, 2.62, m.v. .30.

The better group, academically, is better in the test than the poorer group academically, though the difference is not so marked as in the case of the Opposites Test. The distribution of scores is very scattering, and may be summarized by saying that scores better than 2.5 seconds were made by 56% of the observers in Group I and by 36% of observers in Group II; while scores poorer than 3 seconds were made by 12% of Group I and by 20% of Group II.

Information. Since this test was scored by the number of words new to the observer, the smaller numbers indicate the better performances.

Average number of new words, Group I, 31, m.v. .8.
Average number of new words, Group II, 37, m.v. .78.

An advantage on the part of the superior group in academic standing appears. Fifty-two per cent of the observers in Group I found less than 30 new words; 28% of Group II found less than 30 new words. On the other hand 36% of Group I and 40% of Group II found more than 40 new words, so the difference between the groups seemed to be that there were fewer noticeably good vocabularies in the poorer academic group, but about the same number of noticeably poor vocabularies in the two groups.

The net result of the study is that the Cancellation Test and the Substitution Test do not serve at all to differentiate the best from the poorest students in a group all of whom are up to the standard re-

quired for graduation from college; and that while neither the Opposites Test nor the Analogies Test serves entirely to separate such groups, there is a distinct correlation in the case of both between test performance and academic record. The number of words in the Whipple Information Test which are new to a student is correlated with her academic record: the best records in this test are largely made by students of the best academic standing, but some noticeably poor records are made by such students.

A Note on the Terman Superior Adult Tests, as Applied to Vassar Freshmen

By M. F. Washburn

In the fall of 1917, when the members of the incoming Freshman class were tested, they were put through the Terman Superior Adult Tests. Out of 317 students tested, 46, or 14.5 %, passed all these tests. On 35 of these students there were obtained from their instructors during the year 69 judgments of general ability, made on the Miner Scale, 5 being used to designate the highest and 1 the lowest grade, while 3 indicated average ability. The average of these 69 judgments was 3.36. Eleven of the forty-six had no judgments made upon them. It would appear that our Freshmen 'superior adults' failed to impress their instructors with their superiority, since the average judgment assigns to them about normal ability.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF WILLIAM SMITH AND HOBART COLLEGES.

Communicated by F. M. KUNKEL and G. J. RICH.

I. THE COMPARATIVE SAPIDITY OF HYDROCHLORIC, SULPHURIC AND ACETIC ACIDS.

By L. GIBSON and T. HARTMAN

Corin[1] compared the sapidity of acids by determining the strength of supraliminal solutions with equal tastes, and found that the power to produce taste was not constant either for equal concentrations or for equal numbers of hydrogen-atoms in solution, but that it was greater as the molecular weight of the acid was less, although not in a numerical proportion. Richards,[2] who also equated supraliminal solutions, found that sapidity varied with the degree of ionization of the acid, save that certain weak organic acids were more effective than their degree of ionization would warrant. Kahlenberg[3] used similar methods and came to the same conclusions, noting especially that acetic acid had a stronger taste than was to be expected from the dissociation theory. Kastle[4] obtained like results and suggested that the 'coefficients of affinity' of the acids were effective in producing taste. Crozier[5] made rough determinations of the stimulus-limen for hydrochloric and acetic acids, again finding that the latter acid was more stimulating than could be accounted for by the degree of its dissociation. The stimulus-limen had already been used by Gley and Richet,[6] who found that the taste of the alkali metals depended upon their molecular weights, and by Hober and Kiesow,[7] whose results showed that the limens for salts and for bases remained approximately constant when expressed in terms of ionic concentration. The purpose of the experiments reported here was to apply this same criterion of sapidity, the stimulus-limen, in the case of acids, using a standard psychophysical procedure.

[1] J. Corin, Action des acides sur le goût, *Arch. d. biol.*, 8, 1888, 121ff.

[2] T. W. Richards, The Relation of the Taste of Acids to their Degree of Dissociation, *Amer. Chem. Jour.*, 20, 1898, 121; *Jour. Phys. Chem.*, 4 1900, 207.

[3] L. Kahlenberg, The Action of Solutions on the Sense of Taste, *Bull. Univ. Wisconsin*, 2, 1898, 2ff.; The Relation of the Taste of Acids to the Degree of Their Dissociation, *Jour. Phys. Chem.*, 4, 1900, 33ff.

[4] J. H. Kastle, On the Taste and Affinity of Acids, *Amer. Chem. Jour.*, 20, 1898, 466.

[5] W. J. Crozier, The Taste of Acids, *Jour. Comp. Neur.*, 26, 1916, 453ff.

[6] E. Gley and C. Richet, Action chemique et sensibilité gustative, *Comp. rend., Soc. de biol.*, 37, 1885, 742ff.

[7] Hober and Kiesow, Ueber den Geschmack von Salzen und Laugen, *Zeit. f. phys. Chem.*, 27, 1898, 601ff.

Each of the writers acted as observer for the other. Before starting the main part of the experiment, each went through about two months of preliminary training in order to determine the strength of the solutions which were to be used as stimuli, and to overcome the effects of initial practice. The limens were obtained by the Method of Constant Stimuli, fifty series of five stimuli each being used. Beginning with distilled water in every case, the stimuli differed by .01576 grams per liter in the case of hydrochloric acid; by .0122 grams per liter for sulphuric acid; and by .0155 and .0576 grams per liter for observers G and H, respectively, in the case of acetic acid. The solutions used as stimuli were obtained by diluting more concentrated solutions whose strengths had been determined from their specific gravities.

The stimuli were kept in small bottles fitted with pipettes, which were refilled daily from stock. These bottles were marked according to a code, so that neither the observer nor the experimenter knew at the time of experimentation with which acid she was working, or which were the stronger and which the weaker solutions. The stimuli were administered according to a chance order. The observer seated herself before a sink. The experimenter then took one of the small bottles, pumped the pipette to stir up the solution, filled the pipette to about

TABLE I

TASTE LIMENS
(Grams per liter)

	Hydrochloric			Sulphuric			Acetic		
Observer	*L*	*c*	*h*	*L*	*c*	*h*	*L*	*c*	*h*
G.....	.0284	1.014	35.7	.0386	1.279	33.1	.0640	.867	13.6
H.....	.0358	1.599	44.7	.0551	1.828	33.2	.1296	1.644	12.7

TABLE II.

AVERAGE LIMENS
(Observers G and H)

	Hydrochloric	Sulphuric	Acetic
Grams per Liter.....	.0321	.0468	.0968
Gram-molecules Normal per Liter.....	.00088	.00095	.00161

one cubic centimeter, and handed it to the observer, who opened her mouth and emptied it upon her tongue. As soon as the observer had formed her judgment, she spat out the solution and rinsed her mouth with distilled water, reporting either "taste" or "no taste." After a maximum of three series, experimenter and observer changed places. Experimentation was never begun less than two hours after a meal, or continued longer than an hour. Series for the three acids were carried on simultaneously, so as to equalize any effects of practice.

The limens obtained are given, together with their measures of precision, in Table I. For further consideration we have averaged the results of the two observers. The average limens are shown on the first line of Table II in terms of grams per liter. On the second line of the table, however, the limens are expressed, not in terms

of the strengths of the acids themselves, but in terms of the concentration of hydrogen-atoms in the liminal solution (gram-molecules normal per liter). It will be seen that the hydrogen-concentration is not constant. The order in which the liminal solutions of the three acids come, in regard to this value, which is necessarily the order of their sapidity as related to hydrogen-concentration, is: Hydrochloric, Sulphuric, Acetic. But this is also the order in which they stand with regard to the percentage of ionization in solution.[8] It is to be noted further that the values for hydrochloric and sulphuric acids differ but slightly, which is in accord with the well-known principle that the inorganic acids tend towards complete (and therefore equal) ionization as the solution becomes very dilute. The concentration of hydrogen in the liminal solution of acetic acid, however, is less than twice that of either of the other two acids. Yet the maximal dissociation of acetic acid in the weakest solutions does not exceed 6%, as against a dissociation approaching 100% for the two inorganic acids.[9]

We must conclude, then, that the sapidity of hydrochloric and sulphuric acids seems to depend upon their concentration in hydrogen-ions (concentration in hydrogen-atoms multiplied by the percentage of ionization), but that acetic acid exhibits a stronger taste than its low ionic concentration would justify. These conclusions are a verification, by a more exact procedure, of the results obtained by earlier investigators.

II. The Daylight Mazda Lamp in the Psychological Laboratory.

By Gilbert J. Rich

It has been shown by Brown[1] that artificial daylight produced by means of Gage's glass may be substituted for natural daylight in color-mixing with only slight effect upon the color-equations obtained. The use of some form of artificial daylight presents many advantages, especially because with it one has a light of constant intensity, a great aid in working out the third law of color-mixture. Many laboratories, however, find the cost of Gage's glass prohibitive. More recently the General Electric Company has placed upon the market a new 'daylight' bulb, the "C-2 Mazda," which sells at prices averaging about 20% above the cost of plain nitrogen bulbs of the same intensity. These prices are within reach of the smallest laboratory, which must in any case spend money for electric bulbs of one sort or another, and can now obtain the 'artificial daylight' with only a slight increase in expenditure. In order to ascertain the value of this new light, we have compared color-matches made under its illumination with matches made in natural daylight.

[8] J. W. Mellor, *Chemical Statics and Dynamics*, 1909, 194f.

[9] Mellor, *loc. cit.* We unfortunately did not have at hand facilities for making a direct determination of the per cent of ionization of the solutions which we found to be liminal.

[1] A. J. Brown, Some Uses of Artificial Daylight in the Psychological Laboratory, *Amer. Jour. Psych.*, 27, 1916, 427ff.

The artificial light was furnished by a 150-watt C-2 mazda bulb placed some 3 meters in front of (and above) the color-mixer in a dark room (black walls). The matches used for comparison were made in a room with light walls and two large windows facing east, and only on bright afternoons. In both cases the observer sat 4 meters from the color-mixer. The following papers were used: Orange (O), Blue-Green Shade No. 1 (BG), Green Shade No. 2 (G), and Violet Shade No. 2 (V) of the Milton-Bradley series; and Blue (B), Orange-Yellow (OY), Yellow-Green (YG), Red (R), Black (Bk), and White (W) of the Hering series. Matches were made for eight observers, all of whom had had some practice in color-mixing.

In our first match, G and Y were equated to YG weakened by the addition of Bk and W. In the second combination, O and BG, which seemed to be complementary under the artificial light, were matched to a mixture of Bk and W. They were not, however, complementary in natural daylight, so that it was necessary to add small amounts of B in order to match the gray. And, thirdly, B and R weakened by Bk and W were matched to V.

The averages and mean variations for the eight observers taken together are shown in the accompanying tables, together with the average difference between the results obtained under the two illuminations. There are also included the greatest difference and the least difference for any single observer between the two kinds of lighting, as well as the ratios between the amounts of B and R in the third equation. All measurements are in degrees.

The first match shows that the artificial light which we are testing does no great violence to the hues in the middle of the spectrum. The differences between illuminations are sometimes more, sometimes less, than the *mv* between observers, but are in general comparable to these. A much greater discrepancy is to be found in the blues, as the second of our matches shows. The electric bulb is deficient in blue rays, and therefore gives a yellowish light. A combination of Bk and W on the color-wheel is, then, a very pale yellow instead of a gray, and the seemingly complementary O and BG were actually matched to this yellow under the artificial light. In the natural light, on the other hand, the mixture of BK and W gave a true gray, so that it was necessary to add small quantities of B to the yellowish combination of O and BG before a match was obtained. The third trial shows this same derangement. Since B is less effective under the electric light than in natural daylight, a greater proportion of it in the combination is necessary to match the V. Moreover, since the V contains a large percentage of blue, the paper appears as less saturated in a light which

MATCH NO. 1

		OUTSIDE		INSIDE		
		G	Y	YG	B	W
Natural.....	Average...	312.1	47.9	140.8	192.9	26.3
	Mv.....	3.2	3.2	6.1	6.5	6.5
Artificial.....	Average...	306.4	53.6	145.8	188.3	25.9
	Mv.....	1.8	1.8	5.4	4.0	4.3
Average Difference.....		5.7	5.7	5.0	4.6	.4
Greatest Difference.....		18.0	18.0	18.0	16.2	21.6
Least Difference.....		0	0	0	1.8	0

MATCH NO. 2

		Outside			Inside	
		O	BG	B	Bk	W
Natural.....	Average...	62.7	289.4	7.9	276.1	83.9
	Mv.....	1.1	4.0	4.0	2.2	2.2
Artificial.....	Average...	59.4	300.6	.....	271.8	88.2
	Mv.....	1.4	1.4	.....	1.8	1.8
Average Difference.....		3.3	11.2	7.9	4.3	4.3
Greatest Difference.....		7.2	18.0	14.4	9.0	9.0
Least Difference.....		0	3.6	0	1.8	1.8

MATCH NO. 3

		Outside				Inside	Ratio
		B	R	Bk	W	V	B/R
Natural.....	Average...	162.0	93.2	103.7	1.1	Constant at 360.	1.7
	Mv.....	7.9	7.9	9.4	1.8		.....
Artificial.....	Average...	123.9	61.9	163.4	10.8		2.0
	Mv.....	6.5	4.7	6.8	1.1		.....
Average Difference.....		38.1	31.3	59.7	9.7		.....
Greatest Difference.....		57.6	48.6	72.0	14.4		.....
Least Difference.....		14.4	12.6	32.4	3.6		.....

is deficient in blue rays. As a result, smaller amounts of color (both R and B) were necessary to obtain a match in the artificial light, as the table shows. We must conclude, then, that the Mazda lamp, although dependable through the middle range of the spectrum, gives a light which is markedly deficient in blue rays as compared with daylight.[2]

A comparison of the *mv* between observers under the two illuminations is worthy of note. The average *mv* is 4.7 in the natural, and 3.3 in the artificial light. This difference, though too small to be of real value, is at least indicative of the greater constancy of result that may be obtained when an artificial source of light is used. We have, indeed, found in this laboratory that third-law-matches invariably check when the Mazda light is used, while the difficulty of checking these color-equations in daylight is known to every laboratory worker.

The C-2 Mazda light differs from daylight in too great a degree to permit its use in research work. On the other hand, it is close enough to the natural light to be used to demonstrate the laws of color-mixture in the ordinary laboratory course, for which purpose it has the advantage of constant intensity.

[2] Dr. E. G. Boring has kindly called our attention to a simple demonstration of this deficiency by the principle of the Hering window. If a rod is so arranged as to cast a double shadow upon a white surface illuminated by a slit in a window and a C-2 Mazda lamp, the shadow cast by the daylight is colored yellow by the Mazda while the shadow cast by the Mazda is a good blue by contrast.

We have called the artificial light deficient in blue rays rather than superabundant in yellow rays (which would have the same effect on the second and third of our matches) because the first match showed no important difference in the yellows as between the two lights.

BOOK REVIEWS

A Revolution in Biology and Surgery. Dead Grafts. By CHARLES NORDMANN. (From Revue des Deux Mondes, Dec. 15, 1918). Translated by M. PIETERS.

Although not the most odious of German crimes in starting the war, the most stupid is perhaps the interruption during these years of the progress of human thought, of disinterested research for the beautiful and true, the destruction of those flowers of thought which color the dark pathway of mankind.

Now a singing victory has broken down the barrier of steel and fire raised against the advance of disinterested scientific thought by the homicidal madness of a predatory race. Aside from the material losses caused by the enemy, who can estimate the immaterial ones due to the stupid furor for domination? Who can say how many Pasteurs, Claude Bernards, Henri Poincarés have been taken from humanity in the youth of their genius? Who can measure, among many other minds untouched by death, how much beauty and wisdom might have been born, had not their skill been directed toward the dark tasks of war imposed by the defence of our native soil for more than four years?

But French science is still living, and has been tonified and stimulated by victory. It is preparing to spread once more through the world with clever ideas and fruitful discoveries like those which in the past have made it famous, and enriched all who profited thereby. In this very moment when our soldiers are entering the land of the foe, so arrogant of late, (even as though to show that for France force is but the pedestal of a splendid idea), a great discovery has been made in our country. It is something which will have the largest practical applications; meanwhile, it gives us an astonishing glimpse of the very mechanism of life.

I am referring to the "dead grafts" made by two French scholars, M. Nageotte, professor of histology in the College de France, and M. Sencert, assistant professor of surgery in the Faculte de Médicine of Nancy. We must remember these two names because, as I hope to show briefly in these pages, their recent researches have introduced a new epoch in science.

We know the marvelous works of Carrel, the young Frenchman, who has found in free America, the material means to realize his original ideas. He succeeded in doing this at a time when if he had stayed in this country French scientific bureaucracy would have considered his theories scandalous and anti-administrative. He has magnified the glory of the French name by his splendid works on the culture of living tissues, on grafts, and finally, and more recently, on the treatment of war wounds. I have already had an opportunity to speak in this magazine of his very remarkable work on grafts of various organs, and particularly of vessels preserved living by various means, and notably by a process called by Carrel "cold storage." These researches are world famous and have brought the reward of the Nobel prize to their author.

It is in this field — for in science there is never an edifice so perfect that a new stone cannot be added — that Nageotte and Sencert have

been working, and we are going to see that their discovery amplifies and completes in the most unexpected manner the brilliant results obtained by Carrel. I wish to summarize the discovery in a few words before explaining the origin and methods. It is possible to graft various organs from one animal to another (and man is zoologically an animal) without these organs being preserved living. Furthermore, dead grafts seem to succeed better than living grafts; undergo in the organism where they are fixed transformations which are a veritable resurrection, reviviscence, an astonishing mechanism opening unexpected perspectives in biology.

And now — for nothing is more suggestive than the physiology of the process — I would like to show how the discovery originated.

If a synthetic view is indispensable, to know vital phenomena, or a living being, the analytic study of its elements is still more indispensable. An observer who would look down from ten kilometers above the city of Paris would certainly have a general idea of its dimensions, of its general form, of the disposition of its great thoroughfares and monuments. But, if he wants to know Paris as an active and living organism it is necessary for him to walk down its streets, to see, to hear, to mingle with its inhabitants, in order that he may observe close at hand all its intellectual and artistic treasures, all that makes the life and character of the city. So it is the same when we study a living organism; and that is why histology and the microscopic study of the elementary cells that constitute all living beings enable us to know better what are the essential, the permanent, the general, characteristic, in one word the "life phenomena" of this organism.

Before going farther — to reassure all legitimate susceptibilities — it is necessary to notice that the physical-chemical study of vital phenomena, whatever the result may be, does not free them from a mysterious character that is common to all sensible phenomena. The time is past when Haeckel could candidly believe he had solved the "riddles of the universe" in reducing all phenomena to physical and chemical notions. Even if that were possible and true, even if we could reduce all that exists in a "game" of masses, of electric energy, of chemical affinities, the door of mysteries will not have been closed, because when we go to the bottom of all things physically, we see that we do not know the essential nature of these things we call masses, electric energy, or chemical affinities.

Science never says the final word. It can attempt a classification and isolation of phenomena, but it can never prevent man from further scientific contemplation. All that science can do is to direct its course.

The histological study of living matters and the microscopical study of their anatomical elements and of their cells have been carried out very far in recent times, but perhaps a little too far in a purely morphological sense. Some experimenters in histology, in reaction against the too exclusive static tendency of this science, have wished to apply further the study of dynamic phenomena.

Among these, none are more suggestive in the formation of living tissue than the regenerative tendency which allows us to notice the restoration of flesh and destroyed organs. It is precisely out of the cicatrization of wounds that Nageotte and Sencert have made their splendid discoveries.

I apologize for these somewhat lengthy and technical remarks in introducing our main subject, but this is a question of phenomena of great interest, which although microscopic, it is necessary to under-

stand, in order to discern their great importance; for in science, contrary to what happened in fairy tales, real beauty is chiefly beautiful without a veil and wonder only arises from clearness.

The most important tissue of the human body is the connective tissue, that serves, as its name indicates, as a frame for the various elements of the body, and that constitutes the major part of what we commonly call the flesh, the bones, and the cartillage. This tissue is seen to be constituted, when examined with a microscope of very great magnifying power, of an abundant network of fibres whose diameter varies from a few thousandths of millimeters to some hundredths of millimeters. The network is cut again by a quantity of slits and composes a vast spongy system. In these webs, imbibed with a serosity that is a part of the "milieu interieur" of the human body and that is renewed without intermission we find the few scattered cells, which are the elements of the living protoplasma.

Various theories have been expressed in order to explain the formation of this intercellular network which occupies the greatest part of the connective tissues.

Some people think that this substance is formed by a kind of secretion of the cells that live in it as the compartments of the hive are made by the bees; while others think that it is formed at the expense of the substance of the cells, or rather of their exterior parts, the exoplasma. Now, in studying directly the evolution of scars with the microscope, by means of the classical methods of differentiation and coloration, M. Nageotte and Sencert have been led to the conclusion that the fundamental substance (it is so that they call it with a very appropriate term) of this connective network is formed by direct transformation of the fibrine that appears in the scar. We know that fibrine is the substance that forms in the blood and produces its coagulation when coming out the vessel. Examining a cicatrization in its various phases with the microscope we see that this plexus of fibrine which is born out of the blood plasma is transformed by imperceptible degrees till it presents the characteristics and the disposition of the fibres of the fundamental connective substance. The fibres gather progressively and constitute the connective fasciculi formed by parallel fibres. This uninterrupted metamorphosis is interesting to follow with the microscope.

The conclusion of these experiments, which has been confirmed by others, is that the intracellular substances of the connective tissue are formed directly by coagulation of the blood plasma. In consequence, and this replies to a long debated question, these substances are not more living than the plasma itself. I shall not review here the characteristics of the living substance which are irritability, the ability of reproduction, and of adaption to environment. It follows — and this is true in regard to all the conceptions, however varied, and of all the definitions of life — that the fundamental substance of the connective tissue is not living, while on the contrary the connective tissue cells are living, and this brings us immediately to the following curious conclusion to which we will return. If we consider the great volume and weight of the intracellular connective tissue in the human body we see that, for instance, when the body weighs 70 kg. it does not contain more than 4 or 5 kg. of real living substance.

Going back to my former analogy, the human body is a living thing in the same sense as a big city considered as a whole; the same as the weight and volume of the houses of the city contain but a feeble

fraction of living beings; so our body is only living in a light portion of its weight. This fraction is however by far the most important in its rank and its functions. Hence the solid intracellular substances are not living. If they offer specifically certain appearances of life, by their curious autonomy and their faculty of orientation, it is clearly the result of their reaction to their surrounding conditions, according to M. Nageotte's experiments. We know particularly that the coagulation of albuminoid substances is a very unstable phenomenon and is sensible to mechanical influences. This is easily shown in an experiment by Hardy, where one provokes artificially the coagulation of plexus fibres from a colloidal solution of the white of the egg.

Such are the audacious and suggestive conceptions towards which the most rigorous experimentation has led professor Nageotte, as a real man of science, without any preconceived idea, to follow its Ariadne's thread.

But then, if we frankly abandon any strange preconception and consider only the pure and simple facts, we see in the connective substance only the habitat of living cellular elements. It is this which led Professors Sencert and Nageotte to ask themselves whether this habitat, cleared of its occupants, can receive others. To perform this experiment, there is one means; that is, to kill all the cells in a fragment of connective tissue by dipping it in a solution which kills them and to put this fragment back in the living organism and to observe what happens; in brief, to perform a dead graft.

This is what Sencert and Nageotte have done, and now it remains for us to explain the results they have obtained, which I consider the most striking and the richest in consequences that biology and surgery have produced since a long time.

It is necessary to notice, before going farther, that we have here one of the clearest examples of what hypothesis and experimentation are able to do when they are intelligently united. In his immortal work on "Science and Hypothesis" Henri Poincaré maintained the idea, which had a particular value coming from the greatest theorist of our time, that theories have a value in science so far as they suggest experiments. In this respect, of the various biological and philosophical theories of life, without conjecturing on their real value, it is certain that the physical and chemical doctrine of living phenomena is superior to the vitalistic doctrine, because more than the latter, the former suggests experiments and practical researches. Be that as it may, in the works of Professor Nageotte and Sencert, we see how experiment and the theory reciprocally aid in achievement. Yet Newton had admirably perceived this necessary linking "omnis enim philosophiae difficultas in eo videtur ut a phenomenis investigemus vires naturae, de inde ab his virbus demonstremus phenomena reliqua." Hence the experiments stated above have led to the hypothesis that "the connective substances are inert coagulums, formed in contact with the cells of the organism and serving them as an habitat; thus we may suppose that these substances, borrowed from dead tissues and grafted in the midst of living tissues will be incorporated, without, on the one hand being eliminated like a foreign body, and on the other hand, without being destroyed by the phagocytes which annihilate living substances introduced into the organism.

It would be impossible to explain here in detail the long series of splendid experiments progressively conducted, which in the hands of M. Sencert and Nageotte has shown the foundation of their audacious hypothesis in reality. I will restrain myself — omitting all

the stages lying between, all the difficulties and fruitful gropings—to the indication of the most recent, the most demonstrative, the most delicate and the most daring of these experiments.

The two physicians have grafted, in various conditions, fragments of dead organs in the place of identical fragments taken from the same organs of a living animal.

They have laid open and resected to a length of several centimeters certain extensor tendons of the toes of dogs put to sleep with chloroform. They have replaced the absent substance by the suture of a piece of a dead tendon taken from a dog, previously killed for another purpose. These dead grafts had been preserved more than one month in alcohol. Several weeks after the operation, the dog that had rapidly recovered the integral use of its limbs, was examined microscopically. *"The operated tendons differed in no way from the corresponding tendons of the opposite paw. The dead tendons were a constituent part of the living tendons, they possessed all their morphological and physiological characteristics. Furthermore, no matter how great the magnifying power of the microscope, it was absolutely impossible to distinguish where the tendons ended, and where they started. Finally, and this is the most important discovery the graft that was dead had become perfectly alive again, its connective web had been rehabilitated with living cells that had infiltrated by degrees from the adjacent tendon."*

In the operation performed upon dogs it is possible to see the progressive immigration, the infiltration of living cells in the empty divisions of the dead substance in its different stages. The city was dead, unpopulated but gradually the inhabitants come back and the city revives, resuscitates. Is there anything more wonderful and more fairy-like than this performance displayed in the narrow and yet gigantic field of the microscope which permits us to catch in their unveiled nakedness some of the strangest mysteries of life itself?

These dead grafts, after having been performed with organs of seccondary importance such as the tendons have been applied by the authors to those infinitely more complex and delicate organs, the arteries. They have replaced in animals fragments of carotids by fragments of dead carotids preserved in alcohol and sewn by suture on the sectioned extremities. The grafts, thanks to the technique, which requires great ability, have undergone a complete reviviscence with perfect anatomical and functional results. Still more, grafts of animals of different species have given the same results. A fragment of a sheep artery preserved in alcohol and grafted upon the carotid of a dog has been perfectly successful. It has become a dog carotid while being rehabilitated with canine cells.

Finally, in order to be definitely certain of the respective value of the dead and living grafts, the authors have performed and repeated the following experiment. They have grafted upon the same dog, on one hand, in the place of the right carotid, a dead graft which has been killed in alcohol and on the other hand in the place of its left carotid a living graft preserved by "cold storage." The result which is surprising "a priori," yet absolutely congenial with the theories of the authors, is as follows; the dead graft has been assimilated and functions much more quickly and easily than the living graft.

This evident paradox can be explained in the light of the ideas expounded above. The dead graft is a dead and uninhabited city, ready to welcome immediately the new inhabitants. On the other hand, the living graft possesses in its divisions foreign cells, the life

of which is incompatible with that of the cells of the animal, because, as Carrel has shown in his famous experiments, the living substance of an individual cannot exist in the organisms of another individual, even from an identical species, without producing toxic phenomena. It follows that the animal upon which a living graft has been fixed must first get rid of the intruded cells by means of phagocytes; only then will the animal be able to repopulate the graft with his own cells.

If I dare use the following analogy, the difference between the assimilation of the dead and the living graft is the same as that which exists between the occupation by our troops of an abandoned trench and that of a trench still occupied by the enemy.

In face of these successes, it was possible to pass without danger to the experimentation upon men, that is to say, on the surgical application of the method which from the practical point of view forms the most precious corollary of the new discovery.

It is what the two surgeons have done. After the first operation the success obtained has been wonderful. These results have been recently presented to the "Congres de Chirurgie," to the "Société de Chirurgie" and to the "Académie de Médecine." On a certain number of soldiers whose nerves had been destroyed by splinters of shell M. Nageotte and Sencert have grafted animal nerves preserved in alcohol. The results obtained have been surprising, although in some cases more than one decimeter of substance had to be replaced. The wounded soldiers have regained the absolute use of their limbs. In other wounded soldiers they have replaced in the same way the destroyed tendons by the tendons of dead animals. Perfect results. A wounded soldier had lost all the flexor tendons of the hand, regained flexion of the fingers. When the tissue of the wound was removed, one could see that three or four centimeters of each of the flexors of the fingers were lacking. Nageotte and Sencert replaced the lost substance by suture of eight grafts of dog tendons killed in alcohol. Today, this man is completely cured and moves his hand as he did before he was wounded. One could hardly over-emphasize the importance of these results in surgery.

The graft of dead parts of arteries upon animals has been brought to a successful end. There is no doubt that we will see in the near future this kind of operation performed upon man himself. This graft will have immediately a frequent application of very great importance, for instance, the cure of aneurism which has so frequently condemned a great number of individuals to death. How many lives are going to be saved by these means? Numerous are the other immediate applications of the discovery. Until now, when we had to repair an important loss of osseous substance, for instance in the case of trepanning we made the mending "in vivo" by means of bones taken from the subject himself, by "autoplastie," as they say in the "Faculte." How much easier it is going to be to make the graft by means of a veal or dog bone preserved in alcohol and of which the surgeon will have a provision in reserve in the closet of his operating room? And so for the loss of nose substance, for the "rhinoplastie," so necessary for many crippled of the war, and which a long time before the war certain diseases rendered necessary. In all these domains and in still others the way is now open, and we see all the immediate practical consequences of the suggestive works of Nageotte and Sencert.

If we look now for farther consequences in the field of probabilities the most astonishing vistas are opened before us. These perspectives

follow from what has been said and from a remarkable and unexpected biological fact that has been presented to the "Academie des Sciences" by M. Nageotte and Sencert. While observing histologically and microscopically fragments of the dead carotids grafted by them on dogs, which have been killed sometime after, they have observed and followed in all its phases the following phenomena: The arterial wall is formed as we know by several superimposed layers; the middle layer is constituted in the living artery by special cells called smooth muscular fibres separated by numerous thin and elastic "laminas." In the graft killed by alcohol, these muscular cells have disappeared and only the elastic apparatus is preserved. Now, sometime after the suture of the graft, one can see that this graft starts to be repopulated by muscular cells coming manifestly from the surrounding tissues by transversal progression and not from the extremities of the living artery by longitudinal progression. What it proves is that they are distributed at a given time in the total length of the graft, but more numerous towards the external part of the middle layer. These muscular fibres are common cells of the surrounding tissues, ordinary "fibroblastes" as we say, that transform themselves by degrees as we see with the microscope, into muscular cells with special contexture. There we see an evolution of the common cell that transforms itself into a special cell and has the characteristics of the organ into which it has infiltrated. *This fact overthrows completely current ideas on the specificity and fixity of the various kinds of cells.*

But then, the following consequences may be predicted since different organs, whatever they may be, are formed by a connective web or frame that remains when the organ is killed in alcohol and unites again in the graft with the connective frame of another organism, since on the other hand this dead grafted organ is able by its natural reaction to bring on an evolution of the ordinary cells that come alive into it, as far as to give them the specificity that characterizes them, we can imagine what follows: What would prevent us one day from grafting on an animal or a man whose kidney has to be removed, another kidney "killed" in alcohol which will revive by this "metaplastic cellular" function, at the expense of the surrounding ordinary cells and receive characteristic rhenal cells? What would prevent us from doing it with the heart, with the stomach, and with the brain? The human and social consequences would be tremendous.

What a splendid fiction work in the type of those of Vernes or Wells would it be possible to write on this topic, and is it not stimulating to think that, thanks to the wonderful and profound works of Nageotte and Sencert this fiction of today may perhaps become the reality of tomorrow.

La Psiquiatria Psicologica. By HONORIO F. DELGARDO. Lima, Samarti y Ca., 1919. 22p.

Despite all dire prophecies, psycho-analysis has been steadily extending its influence so that upon the northern half of the American continent medical doctrine, sociological and historical teaching, and even literature has felt its imprint. Now, as a review of Senor Delgardo's pamphlet, which purports to be an epitome of a larger volume on the subject, shows, South America, too, is receiving its interpretation of the new psychology. Indeed, it is claimed that the

work of the Spanish scientist Pinel is in many respects a precursor of the psycho-analytic methods and therapy.

While the scope of the article is too limited to permit of more than the briefest outline of the psycho-analytic theory, it shows very conclusively the author's comprehension of the science, and his familiarity with German, French and English authorities, as evidenced by quotations from Freud, Jung, Silberer, Ferenczi, Janet, White, Burrow, Jeliffe, Hall, et als. Although it contains little original material, it is to be recommended to those who desire a summary of the essential points of psycho-analysis in the Spanish language.

PHYLLIS BLANCHARD.

BOOK NOTES

The Child's Unconscious Mind. By WILFRID LAY. New York, Dodd, Mead and Co., 1919. 329 p.

The chapters are: The Unconscious Factor, Interplay of Conscious and Unconscious, The Partial Trends, The Mechanisms, The Aim of Education, Resistance and Transference, Emotion, and Conclusion.

This is the most practical attempt by an experienced teacher to bring Freudian principles into modern pedagogy. It is written with no less attractiveness than the author's previous work, "Man's Unconscious Conflict." It is popular, and brings the unique and epoch-making Freudian conceptions of childhood and youth to bear in the schoolroom and also in the home. It seems to show that every child, normal as well as abnormal, should undergo some kind and degree of psychoanalysis, not of course in the original Freudian sense but psychoanalysis here used as an instrument to teach the child self-knowledge and to enable those who have to deal with him to adapt their methods to the nature and needs of the individual child. Perhaps no single work better illustrates the modern tendency of pedagogy to recognize and stress individuality.

Studies in Psychology. Edited by E. A. KIRKPATRICK. Boston, Badger, 1918. 194 p.

The topics treated are Habit, Memory, Imagination, Attention, Concepts, Perception, Feeling and Will. The different chapters are the results of experiments in teaching psychology in an inductive, expressive and cooperative way to divisions of a class of beginners in a normal school. After the subject had been worked up in a very interesting way by which all their reading and observations were pooled, the author guided them in expressing in writing what they had observed and classified from first-hand experience. The conclusions are very largely practical. It is a very suggestive book, and is of especial interest to teachers.

The Twentieth Plane. By ALBERT DURRANT WATSON, M. D. Phil., G. W. Jacobs, 1919. 312 p.

This book is dedicated to the heroes of war and shows that there is no death. The twentieth plane is astral, and Agassiz, Coleridge, Wordsworth, Ingersoll, and many other shades are invoked to describe their life on the lofty plane of this order. The first twentieth-plane personality to be studied is, as is proper, the author's mother, who tells him: "We are preparing to receive thousands from the earth plane, and we are resolved to give our best to those souls just arrived." The author is not a preacher but a reporter, and he has messages from masters of prose and of poetry, of art, of eloquence, of statesmanship, etc. He discusses life principles, the quest of reality (in no Freudian sense), another hour with great thinkers, and the realizations of God. The author is a Fellow of the Royal Astronomical Society of Canada and President of the Association for Psychical Research of that country.

Crime and Criminals. By CHARLES MERCIER. (Introduction by Sir Bryan Donkin). N. Y., Holt, 1919. 290 p.

A subtitle of this book is: Jurisprudence of Crime Medical, Biological, and Psychological. It is certainly a valuable contribution to the study of criminal jurisprudence, and the author tells us at the outset that, with the exception of logic, there is no subject on which so much nonsense has been written as on the subject of criminality and criminals. The problem is to apply principles derived from the larger field of conduct in general to crime, so that the first chapter contains much material found in his previous book on *Conduct*. His chapters treat of the factors, the physiology, the nature, the kinds of crime; pride, family, and racial crimes; kinds of criminals; and the prevention, protection, and punishment of crime.

Deficiency and Delinquency. An interpretation of mental testing. By JAMES BURT MINER. Baltimore, Warwick and York, 1918. 355 p.

The work contains twenty-two tables and nine figures, and the book is divided into two parts, (1) practical, and (2) theoretical considerations. Under the first the chapters treat of the function of a scale in diagnosis, the percentage definition of intellectual deficiency, what percentage is feeble-minded, adapting the percentage definition to the Binet-Simon, testing deficiency, checking the Binet diagnosis by other methods, school retardation among delinquents, a comparison of school tests and the Binet tests, etc. Part II treats of the theory of the measurement of mental development and quantitative definitions of the border-line life. An interesting bibliography of tested delinquents is appended.

The Psychology and Pedagogy of Anger. By ROY FRANKLIN RICHARDSON. Baltimore, Warwick and York, 1918. 100 p.

This is number 19 of the Educational Psychology Monographs edited by J. Carleton Bell. The substance of it was originally presented as a Clark thesis. After an introduction, the author considers in four chapters the following topics: Mental Situation Stimulating Anger, Behavior of Consciousness, Disappearance of Anger, and Educational Function. He appends an interesting bibliography that is well up to date. The merit of this work consists in the many original records kept mainly by a number of friends of the author of their own experience with anger, not so much of clonic outbreaks as of the fainter irritations and mild chronic indignation that is so common to some temperaments.

Orthogenic Evolution of Pigeons. Posthumous Works of CHARLES OTIS WHITMAN. Edited by HARVEY A. CARR. (Preface by Oscar Riddle). Washington, The Carnegie Institution of Washington, 1919. 161 p.

This is the report of a very elaborate work of one of our most accomplished American biologists which was left unfinished at his death. Had it been complete, it would have given ample room to comparative behavior and voice as indices of species relationship. Only the manuscript on the behavior of pigeons was near to a final form. The chief topics here treated, each in a chapter, are the mating period, the incuba-

tion period and its behavior, feeding and care of the young, defective cycles, the genetic standpoint in the study of instinct, the reproductive cycle, voice and instinct in pigeon hibernation and phylogeny, the homing instinct, other instincts, and instinct and intelligence.

Squibb's Materia Medica: A complete Alphabetical List of the Squibb Products. Published by THE MEDICAL DEPARTMENT, E. R. SQUIBB AND SONS. N. Y., 1919. 544 p.

The late Edward R. Squibb was a true gentleman of the older school, the hundredth anniversary of whose birth was celebrated on July 4, 1919. After a brief practice of medicine in Philadelphia, he turned his attention to pharmaceuticals and chemicals. His Materia Medica is largely a conspectus of the products of the Squibb laboratory. He also has attempted to standardize biological products as trustworthy reagents. The drugs listed and described in this book number several thousand.

Thirty-second Annual Report of the Bureau of American Ethnology, 1910-1911, Washington, Government Printing Office, 1918. 819 p.

The main content of this book, in addition to the official reports of the Bureau, is a paper by J. Curtin and J. N. B. Hewitt on "Seneca Fiction, Legends, and Myths."

Annual Report of the U. S. National Museum, 1918. Washington, Government Printing Office, 1919. 175 p.